THURSDAY, JANUARY 25, 1883

THE THIRST FOR SCIENTIFIC RENOWN

Few students of science can fail to feel at times appalled by the ever-increasing flood of literature devoted to science and the difficulty of keeping abreast of it even in one special and comparatively limited branch of inquiry. Were merely the old societies and long established journals to continue to supply their contributions, these, as they arrive from all parts of the country, and from all quarters of the globe, would be more than enough to tax the energy of even the most ardent enthusiast. But new societies, new journals, new independent works start up at every turn, till one feels inclined to abandon in despair the attempt to keep pace with the advance of science in more than one limited department.

One of the most striking and dispiriting features of this rapidly growing literature is the poverty or worthlessness of a very large part of it. The really earnest student who honestly tries to keep himself acquainted with what is being done, in at least his own branch of science, acquires by degrees a knack of distinguishing, as it were by instinct, the papers that he ought to read from those which have no claim on his attention. But how often may he be heard asking if no means can be devised for preventing the current of scientific literature from becoming swollen and turbid by the constant inpouring of what he can call by no better name than rubbish !

Some sciences seem to be specially exposed to inundation of this kind. Geology lies exposed to it in an unusual degree. Popular in its subject, and capable of ready apprehension as to its general principles, this department of science allures the outsider into its precincts, where he too frequently soon arrives at the belief that to have read a geological book or two is to become a geologist. This belief would be harmless enough, did it not speedily bear fruit in "papers" communicated to scientific journals, and stamped with all the enthusiasm and crudity of a beginner. On no account should any check be placed on the legitimate ambition of the youngest aspirant after scientific renown. But we venture to think that the common precipitate publication of his earlier efforts is not a legitimate ambition ; but on the contrary is really an injury to himself and a positive hindrance to the progress of the science which he no doubt loyally desires to serve. It too frequently happens, moreover, that his first efforts are directed to the pleasant task of discovering flaws in the work of those who have preceded him. And of course the more eminent these predecessors, the greater his credit in setting them right. Let him take to heart the old maxim, *Festina lente.* The longer he delays his appearance as an author, and the wider he meanwhile extends his practical experience of nature, the more tolerant will he become of the work of others and the less overweeningly confident of his own. In no department of natural knowledge can a real acquaintance with the subject be gained save as the result of prolonged study.

These reflections have been suggested on the present occasion by the perusal of a pamphlet which exhibits in the most glaring way the tendency on which we have animadverted. It is devoted to the announcement of a brand-new theory of the origin of Fingal's Cave.[1] Curiously enough this is not the first time that the basaltic colonnades of Antrim and the Scottish Isles have furnished the text for teaching the most arrant nonsense. Nearly forty years ago a sailor, familiar with tropical bamboo jungles, started the idea that columnar beds of basalt are neither more nor less than petrified growths of bamboos. After vainly trying by occasional newspaper letters to find supporters, he seems to have given up the struggle against the blind prejudices of geologists. In 1864, however, his views were taken up by another writer yet more outrageous, who published a pamphlet of nearly 100 pages, entitled "The Giant's Causeway once Bamboos," and in supporting his dogma ran a tilt at religion, science, tradition, history, in short at everything that happened to suggest itself in the course of his incoherent and erratic pages.

The author of the pamphlet cited below, Mr. F. Cope Whitehouse, M.A., &c., is obviously a man of original genius, and is resolved that the world shall know it. In the summer of 1881 he seems to have come with the mob of tourists that annually makes a pilgrimage to the coast of Antrim. But instead of merely submitting to be led through the usual route by the inevitable and inexorable guide, he boldly separated himself from the gaping crowd, and proceeded to meditate. To his rapid mental vision it was soon apparent that the caves of that coast-line, instead of being the work of the sea, as ignorant mankind has hitherto believed, have been hollowed out by human hands. At once he could perceive the intimate relationship of Gothic doorways, ancient civilisation, mediæval castles, Irish manuscripts, and the "Kelto-Iberian, Wend or Phœnician" race. The narrow sea-worn gullies of Antrim being thus shown to have been ancient harbours, his eye looked northwards to the dim blue Scottish Isles, and his venturous imagination at once demanded whether that world's wonder, Fingal's Cave, might not after all be merely a piece of man's handiwork. To state the question was in effect to answer it affirmatively. Nevertheless that no unsympathetic geological Philistine might blaspheme, our courageous hero sailed for those far islands of the west, saw Staffa with his mortal eyes, and found how well his prophetic intuition had divined the secret of that weird place. We almost envy the thrill of satisfaction that must have vibrated within him as he proudly felt that scientific observers for a century past, from the days of Sir Joseph Banks down to our own time, had one and all missed the true meaning and history of the Caves of Staffa, and that it was reserved for him, casual visitor as he was, to lift the veil and reveal the mystery to our astonished gaze.

Knowing well the type of which Mr. Whitehouse is a fresh and most characteristic example, we hardly require his assurance that as soon as his eye lighted on Staffa his "conjecture received strong and unexpected confirmation. It was subjected to rigid examination ; it was strengthened by opposition." Of course it was. Then, like all similar enthusiasts, his soul could find no rest until he had proclaimed the truth to the nations. Returning to America, he found an opportunity of enlighten-

[1] "Is Fingal's Cave Artificial?" A paper by F. Cope Whitehouse, M.A. (New York: Appleton and Co., 1882.)

ing the darkness of the American Association for the Advancement of Science, at its meeting in Montreal in August last. A few weeks later he proclaimed the great discovery to the Academy of Sciences of New York. But these were limited audiences, though composed, no doubt, mainly of scientific men on whom as yet the true light had never shone. It was absolutely necessary to appeal to a wider, and possibly more sympathetic public. Accordingly he published his views in the December number of the *Popular Science Monthly*. But he was still unsatisfied, till at last he conceived the noble idea of combining the spread of truth with promoting the erection of the Statue of Liberty enlightening the World. He hired a theatre in New York, gave an account of his astonishing observations, charging a dollar a head for admission, and stated that the proceeds of his "matinée" were "to be devoted to the pedestal of the colossal statue." Let us hope that the sum realised was worthy at once of the great truths proclaimed by the lecturer, and of the national object to which it was to be given. Future pilgrims to the colossal Statue of Liberty will piously scan the pedestal, searching for the stone that shall hand down to the far future the name of the illustrious seer who could brush away the tangled cobwebs spun by a century of scientific babblers, and pierce into the true meaning of the Cave of Fingal.

Mr. Whitehouse has published so far only an abstract of his address, but he has had it well printed with good illustrations, and seems to have generously scattered copies of it broadcast over this country. It was not of course at all necessary that he should communicate the steps of the reasoning by which he was led up to his great discovery. And he has considerately refrained from troubling the world with such unprofitable details. Still one cannot help trying to follow the mental process by which an epoch-making deduction has been reached. We get from the abstract glimpses of the way in which the received explanation of the caves of Staffa collapsed at the touch of Mr. Whitehouse's genius. He visited the scenery in calm summer weather. From Staffa he could see the great sweep of the Mull cliffs to the east and the broken rampart of islands all round the rest of the horizon. As the smooth sea mildly heaved along the base of the basaltic colonnade, he could easily persuade himself that Staffa must be a "singularly sheltered," "land-locked" island, and that "the force of the breakers is inconsiderable." How absurd then must it have appeared to him to attribute to that placid lake-like water the power of hollowing out caves in a rock so obdurately stubborn as basalt! Moreover, he could see no reason why the sea, supposing it gifted with such power of erosion, should have chosen the places where the caves actually occur. And his inability to find this reason satisfactorily disposed of any possible action of the waves. Not only so, but from his stand-point at Staffa his clear vision could take in the whole coast-line of Scotland, and he made the further important announcement, which will doubtless for ever silence our northern geologists, who believe in the geological power of the waves, that "there are very few hollows worn by the sea in the Scotch coast!" Having cleared away the fictions of so-called scientific observation, he could apply the much more reliable conjecture which his glance at the Giant's Causeway had evoked in his own mind. To his trained eye the caves of Staffa were obviously artificial. Oracularly he tells us that they are "strikingly Phœnician." "No such Gothic arch was ever formed by nature. No natural cave has an entrance higher than the interior." (!) Lastly, from the end of Fingal's Cave you can see the Hill of Iona rising against the sky, consequently the cave must have been excavated by men who lived on Iona. This final argument must be regarded as a crushing answer to those who have recklessly talked of the power of the waves in these regions. On what conceivable grounds can we suppose that the sea would make a tunnel, from the end of which the Dun of Iona would be visible?

Perhaps some unconvinced outsider may be tempted impertinently to ask for what object such stupendous excavations could have been devised by any civilisation, whether ancient or modern—excavations always swilled with the surge, often unapproachable for weeks together, and in which, even in calm weather, unless care be taken, a boat is liable to have its bottom knocked in. Of such questions Mr. Whitehouse very properly takes no notice. Again, we were in the belief that the religious community of Iona had been an eminently peaceable folk, liable to invasion by pirates from the sea or marauders from the mainland, but with little more to oppose in the way of defence than the prestige of their sanctity. But this conception also is now found to be false. We learn, on the same reliable authority, that they were a warlike race, quite able to look after themselves. It appears that Mr. Whitehouse has shown "the strategic importance of Staffa, and the probability that the wealth and refinement of Iona were due to the protection it afforded." He will have no difficulty in further proving that the traditional picture of the saintly Columba is a mere myth, and that the abbots of Iona possessed an army and navy, made war on heathen Pict and savage Scot, curbed the fury of fiery Norseman, and employed their gangs of prisoners in tunnelling the caves of Staffa!

There is a hollow among the rocky knobs that rise inland from the summit of the cliffs above Fingal's Cave. We would fain place Mr. Whitehouse there on a day when the gathering clouds have blotted out Ben More, when thick mists are driving along the opposite precipices of Gribon, when the Treshnish Isles grow fainter every moment in the western sky, when even Iona, that lies so near, is fading into the general gloom, and when the wild moan of the rising south-western gale among the crags around is answered by the hoarse clamour of the surge below. We should like to keep him there while the gale rapidly increases, breaker after breaker careering madly forwards with foaming crest from under the pall of driving rain that hides the sea, dashing into every creek and cave, rushing in sheets of green water up the face of the crags, and pouring back in hundreds of yeasty torrents into the boiling flood. We would ask him still to stay till the storm has reached its height, that he might feel the solid island shake under his feet, that he might see the sheets of water, foam, and spray thrown far up into the air, that he might hear the cannon-like thunder of the shock as each billow bursts into the Cave of Fingal. He would return a wiser (and a wetter) man, and would regret that in a rash moment he had published some childish nonsense

about man having excavated sea-worn caves, and had expressed an opinion about the power of the sea of which he would then feelingly admit that he had been profoundly ignorant.

The pamphlet here noticed did not in itself deserve consideration in these columns. We have made use of it as a type of publication painfully frequent in the literature of science. If in exposing its characteristics we deter any rash and immature aspirant for fame from at once rushing before the world with what he conceives to be his discoveries, we shall have done a service at once to him and to science.

CINCHONA PLANTING

A Handbook of Cinchona Culture. By Karel Wessel van Gorkom. Translated by Benjamin Daydon Jackson, Sec.L.S. (London: Trübner, 1883.)

Die Chinarinden in Pharmakognostischer Hinsicht dargestellt. Von F. A. Flückiger. (Berlin: R. Gaertner, 1883.)

THE rapid extension of cinchona planting in India, Ceylon, and Jamaica will make a translation of Van Gorkom's account of the methods of cultivation and harvesting pursued by him, as Director of the cinchona plantations belonging to the Dutch Government in Java, useful to many who propose to turn their attention to this profitable industry. At present intending planters in British possessions have had little beyond Dr. King's Manual of Cinchona Cultivation (1876) to serve as a guide. In Ceylon the planting community includes many men of first-rate ability, and the singularly energetic journalism of the island speedily ventilates for the common good any fresh idea or point of practice in planting procedure.[1] Indian planters share the benefit of this, while Jamaica has the advantage of possessing in Mr. Morris, a director of its botanical department, who has carried to the West Indies an intimate knowledge of all that is being done in Ceylon. It is not very probable that those who are at present occupied in cinchona enterprise in British possessions will glean much from Van Gorkom's book. Still such a manual will not be without its use for those who have everything to learn about the matter, and, as will be seen, it cannot fail to be interesting to those who watch from an independent point of view the economics of the subject.

The book is handsomely printed and got up—too handsomely, indeed, for workmanlike use, for which its size, that of a small folio, seems particularly unsuited. We must too make a serious protest as to the style of the translation, which, we think, cannot be considered tolerable, even with every allowance for "seeming inelegancies" which Mr. Jackson pleads for in his preface. Take, as a sample, the first sentence which caught our eye:—

"If we trust that this excellent opportunity for fruitful comparisons shall lead to unfettered judgment, still more do we look for, from the impressions received and the enlarged field of view, the scientific work carried on, which has so long been in hand, and most certainly with great completeness and undisputed knowledge of material, will indicate our present standpoint in the domain of quinology" (p. 264).

[1] Of T. C. Owen's Cinchona Planter's Manual, published at Colombo, we know nothing beyond the name.

Now it is quite certain that this is not English, and we have some doubts whether it really conveys any meaning at all. But at any rate we would ask what is the use of translating in this way a work the purpose of which is not literary but essentially utilitarian. There seems, in fact, to be a deep-rooted superstition about the value of so-called fidelity in translating books of mere information. In rendering a foreign language as a philological undertaking, it is often desirable to sacrifice, to some extent, style and form, in order to convey as nearly as may be, the exact force of each word and of each turn of expression. But where, as in a technical treatise, it is only the context we care about, it is exasperating to find the translator exhibiting a would-be scholarly care over the exact reproduction of the vehicle. All we want him to do is to master the meaning and give it to us in clear, straightforward English.

Having said so much by way of criticism we may indicate a few points which we think will be interesting even to some who are not colonial readers of NATURE. A hundred of the three hundred pages of which the volume consists is given up to historical matter regarding the history of *Cinchona* and the development of its culture in Java and in British possessions. All this is an oft told tale, and contains little that will not be found in Mr. Markham's Peruvian Bark (reviewed in NATURE, vol. xxiii. pp. 189-191). An exception must be made, however, as to the interesting account of the commencement of cinchona cultivation in Bolivia. The existence of this enterprise was known, but we have not met with any previous account of it. The Dutch Consul-General reported to his Government:—

"The great event in the agricultural region of Bolivia is the planting of the Bolivian cinchona forests, of which an earnest beginning was made in 1878. . . . The river Mapiri, in the province of Larecaja, department La Paz, has been the centre of the movement, and already the young trees of two years' growth, may be reckoned at from four to five hundred thousand" (p. 17).

Doubt is, however, expressed whether the planting will be maintained in the face of labour difficulties and a possible fall of prices in consequence of increasing exports from the East Indies.

Modern cinchona enterprise in Java has aimed at the production of barks rich in quinine. With the lucky purchase from Mr. Ledger in 1865 of a packet of seeds of the now well-known *Cinchona Ledgeriana*, the Dutch "cinchona culture of the future has entered upon an entirely new phase" (p. 77). About 20,000 of the seeds germinated in Java, and first and last Mr. Ledger received about 24*l.* from the Dutch Government, and "was therewith well content" (p. 91). Fortunately the greater part of the seed originally imported was purchased by a well-known Indian planter, Mr. Money, and some of it seems by private channels to have found its way to the Government plantations in Sikkim. The Dutch having got this valuable kind seem to have managed it with extraordinary intelligence and skill. Men like De Vrij, Moens, and Van Gorkom were well-trained European scientific men and competent chemists. Their object was by continuous selection, controlled by repeated analyses of bark made on the spot to obtain races of *Cinchona Ledgeriana* richer and richer in quinine, and it is a matter of general

notoriety how well they have succeeded.[1] It is the part of Van Gorkom's treatise dealing with this matter which cinchona planters will be grateful to Mr. Jackson for putting within their reach. Two conditions of success in harvesting good seed are insisted upon.

"For seed saving, the handsomest strongest trees are selected, and especially amongst those whose superior value has been ascertained by chemical examination. Disappointment is inevitable where the eye and botanical characters alone are made use of and trusted to; *the whole issue depends upon the certainty that varieties rich in quinine are exclusively propagated.*

"The choice being made there is something else which must not be neglected; it further behoves us to be perfectly sure that the tree is not fertilised with foreign pollen, that is to say, pollen of an inferior tree or variety" (p. 136).

The last condition cannot be insisted upon too forcibly, notwithstanding that competent botanical opinion can be quoted against it. In their home in South America the different species of *Cinchona* are localised at different points of the Andine chain. Geographical isolation keeps them uncrossed. But where they are brought together in one plantation they hybridise freely. *Cinchona robusta*, which is now widely diffused in India, undoubtedly first originated in Ceylon as a cross between *C. officinalis* and *C. succirubra.*

The aim of the Dutch Government being to produce a commercial bark of high quinine-producing quality, in which they have met with extraordinary success, Van Gorkom is somewhat disposed to criticise the different policy which has been pursued in British India:—

"The Bengal Government . . . makes its cinchona culture serviceable before all things to the wants of its population, and thus only asks itself, how the people and army may be provided with febrifuges on the most advantageous terms" (p. 229).

He sets against this the "well-known fact that not one half of the alkaloids possessed by the raw material are obtained, the greater part being lost.' Even supposing, however, that things are as bad as this, and not susceptible of improvement, it is still arguable whether, looking at the cheapness with which red bark can be grown and converted into a febrifuge—the usefulness of which is incalculable—the theoretical waste is a matter for the present of much consequence. But it is unreasonable to suppose that the Bengal methods of extraction are not susceptible of improvement, though they will probably never reach the standard practicable by more expensive methods in Europe. But the objection of wastefulness must be measured by the circumstances. The proprietor of an estate in England who, with a view of bringing a portion of his park into tillage, began by burning the timber upon it, would be considered a madman. But this is habitually done in clearing a piece of tropical forest for cultivation, and as it is not easy to see what else could be done, a complaint as to the waste would not be much to the purpose. It might have been expected that Van Gorkom's sympathies would have centered in the quinine-producing yellow barks which are for the moment most in favour. This, however, is largely due to the unreasonable importance which is attached to quinine over other cinchona alkaloids. Van Gorkom does not share this prejudice:—

"The conviction has more and more gained ground, that good cinchona barks judiciously applied, frequently do not merely rival quinine, but even surpass it in useful effect" (p. 212).

This point of view is exceedingly important with regard to red bark (*C. succirubra*), which is the easiest of all species to cultivate.

"There is no cinchona bark richer in alkaloids, and though *C. succirubra* is not suitable for the preparation of quinine, because it can only be treated with trouble and much expense, yet it has a preponderance of the secondary alkaloids. No better material for pharmaceutical purposes is known, and on that account its propagation is desirable from every point of view" (p. 100).

High class yellow barks are by no means free in their growth or particularly easy of cultivation. It has been found useful to graft them on *succirubra* stocks, and the practice has been adopted in Sikkim and Ceylon; Van Gorkom gives a useful account of the method adopted in Java.

We must refrain from pursuing many other points which these pages suggest. Two of the concluding chapters deal with the possible synthesis of quinine and the commerce of the barks. As to the former the author has little doubt of success. Two isomerous bodies, chinoline and chinoleine, are known, of which the former is obtained by the distillation of coal tar, the latter by that of quinine. This is thought then to be the clue by which the construction of quinine from coal-tar products will be eventually achieved. But he takes comfort for cinchona planters from two considerations. One is that the synthesis of a vegetable substance when effected does not always result in its practical commercial replacement. The synthesis of alizarine it is found after all does not give the dyer quite what the madder plant gives him. Artificial quinine then may—if ever produced—prove only of interest to the chemist. His other consolation is based on what is said above—that pharmacy can never dispense with the total aggregate extracted products of bark, and the day may be regarded as indefinitely distant when the chemist will be able to replace these any more than such complexes as the contents of our tea- and coffee-pots.

As to commerce it is interesting to learn that London is the most important market for bark, and Paris next. We fear, however, from statistics obtained from another source, that this country has no corresponding lead in the production of the manufactured products, only about 10 per cent. of the quinine of the world being made in England. Yet Van Gorkom states emphatically that "the consumption at the present day of cinchona and its alkaloids, merely represents a paltry fraction of the quantity which will be required to satisfy the prescription of humanity in every country, and among all classes and races of men" (p. 236).

We have left ourselves but little space to notice Prof. Flückiger's handy and concise work, which, though of importance to cinchona planters, is primarily a pharmaceutical study of the subject. The bark of *Cinchona succirubra* has been recently adopted as the official bark of the German Pharmacopœia—a fact of no small importance to planters in British possessions, when it is remem-

[1] Acknowledgment must be made of the striking liberality with which the Dutch Government officials have always placed what they could spare of their selected seed at the disposal of planters in other countries.

bered how enormous is the extent of its cultivation in their hands. It is this fact which has won it its official status, as though poor in quinine its quality is tolerably uniform, and being easily grown its supply can always be depended on. Prof. Flückiger gives a figure of the plant as well as of *Cinchona Ledgeriana*—the quinine bark *par excellence*—and of *Remijia pedunculata*, one of the sources of the *Cinchona cuprea* which has of late years been poured into European markets from South America.

MARINE SURVEYING

A Treatise on Marine Surveying. Prepared for the use of younger Naval Officers, by the Rev. J. L. Robinson, B.A., Royal Naval College. (London: Murray, 1882.)

THIS book has been written apparently with the view of enabling young naval officers to cram themselves sufficiently to pass the examination in surveying at the Royal Naval College, and it must be conceded displays considerable industry on the part of Mr. Robinson, who has evidently taken pains to go through the examination papers on surveying from their commencement, to see what questions are usually asked, and in what form they could be best answered; and has besides consulted a large number of works bearing on surveying, a list of which he gives at the commencement of his treatise; but we confess we are much disappointed that with such excellent materials, so poor a result should have been produced, for, with the exception of the chapter on tides, which in its way is excellent, the work is of very little value, and rather reminds us of that treatise of—

> "The young lady of Buckingham
> Who wrote about geese and stuffing 'em,
> But found out one day
> She'd neglected to say
> A word in her book about plucking 'em."

Mr. Robinson says in his preface "he has had no intention to write a handbook for the use of the practical surveyor," and that "such an intention might fairly be regarded as an impertinence in one who has never been engaged in the practical work of the profession," but that he has had rather "the examination room and its requirements before him." But did it not strike Mr. Robinson that the practical surveyor selected to examine the candidates might ask questions upon which he has neglected to touch, and that consequently his treatise might fail to ensure success in the "examination room," notwithstanding the valuable hints he has received from Staff-Commander Johnson and his friend of great experience as a first-class surveyor?

The first chapter consists of extracts from Admiralty publications, but we recommend the officers at the college to consult those publications for themselves, more especially the Admiralty list of abbreviations, as the illustrations in this work give a poor representation of the symbols and signs used by the draughtsman and engraver.

The second chapter, on the Construction and Use of Scales, and the sixth, on Instruments, are derived principally from Heather. Here again we prefer the original to the copy.

The third chapter, on Laying off Angles, merely contains a brief description of the methods of plotting angles by chords with a small radius. On this we would remark that the real value of plotting angles by chords consists in their being plotted with long radii, as any practical draughtsman could have informed the author.

The fourth chapter is a most elaborate analysis of the method of Fixing a Position by Angles, &c. Surveyors take sextant angles, principally, to fix their positions when sounding, and invariably use the station pointer for that purpose; this chapter therefore seems to us to be firing a 12-ton gun at a sparrow.

The fifth chapter, on Charts and Chart Drawing, is rather a description of the method of map construction, and contains some mis-statements. Evidently Mr. Robinson is not well acquainted with the mode of constructing charts at the Admiralty or by surveyors, as he states in one paragraph that circumpolar charts are usually constructed on the gnomonic projection, whereas we are not acquainted with one Admiralty circumpolar chart on this projection. It is true a diagram is published to facilitate the practice of great circle sailing but no circumpolar chart.

The fact is all marine surveyors project their work on the gnomonic projection, and as the smallest scale in use is an inch to a mile, it is evident that the errors of this projection are very slight, as the largest sheet of paper that can be worked at conveniently is about six feet square. When the original surveys arrive at the Admiralty the Hydrographer decides in what form they shall be engraved and published. If the surveys are plans of harbours, they are usually published on the gnomonic projection (as they were originally drawn); if the survey is of a coast, or to be incorporated in a coast, or general sheet, it is transferred to the mercatorial projection, for which the meridional parts of the spheroid are used. Charts of the circumpolar region are however published on an arbitrary projection, in which the parallels of latitude are drawn as concentric circles at equal distances from the pole.

Chapter seven is on Base Lines. Now base lines are principally of use to the marine surveyor as the quickest method of starting his work, which, when it extends over a large area, almost invariably depends eventually for its scales on astronomical observations.

Mr. Robinson states that it is impossible to fix the position *exactly* by means of a sextant. Here we must differ from him, and will give one instance to the contrary. When the question of the boundary between the United States and British North America was decided, and the 49th parallel was fixed on, Admiral Sir George Richards then in command of H.M. surveying vessel *Plumper*, at Vancouver's Island, was directed to ascertain the position of this boundary line on the western seaboard of North America. This he did with a sextant, and buried a mark in the ground on the position of the 49th parallel as ascertained by himself. The Americans sent a party for the same purpose with a zenith sector and altazimuth and when they had fixed the position of the 49th parallel by these means, the difference between the two results was found to be less than 100 feet! It is of course as well that nautical surveyors should know the various methods employed in obtaining accurate bases for geodetical measurements, but for marine surveying the same nicety is not required as in measuring the arc of a meridian, and it cannot be too often impressed on the mind

of the aspirant in surveying that over accuracy (that is such minuteness as cannot be represented on paper) is loss of time.

The eighth chapter is on Triangulation, and is more worthy of attention than those preceding. If we remember rightly, there was only one three-feet theodolite used in the Ordnance Survey of Great Britain, which instrument is the property of the Royal Society. In fact, so far as we are acquainted, there are only two three-feet theodolites in existence, Ramsden's, and another used in the Great Trigonometrical Survey of India.

The observation that very distant stations are generally observed at night is now subject to correction, as the heliostadt has rendered it quite as easy to observe by day, in fact, in some of our marine surveys, triangles whose sides were 60 miles in length, have been obtained with these instruments, and an eight-inch theodolite, with the greatest ease. With the eight-inch theodolite, and by means of repeating the main angles round the circle, very accurate results may be obtained; and the spherical excess has to be allowed for, and deducted, in order to make the triangles plane, for in all nautical surveys the chord of the arc is used both for calculation and plotting.

The ninth chapter, on Levelling, contains not only an account of levelling, but also of obtaining heights by means of the barometer and thermometer, but totally neglects the method in general use by surveyors, viz. by angles of elevation and depression with a theodolite. For travellers the barometer and thermometer give an approximation of the elevation, which is exceedingly useful in an unsurveyed district. For work requiring extreme accuracy careful levelling is required, but for nautical work the principal use of the level is to ascertain the exact difference between the zero of the tide gauge and some permanent mark on shore, so that a fixed datum can always be referred to for reduction of soundings in future. The heights of hills are almost invariably obtained by angles of elevation and depression, and the results so closely approximate to the truth that it is waste of time to do more, unless results less than five or six feet in error are absolutely requisite.

The tenth chapter is on Tides, and is, as has been before mentioned, well worthy of perusal, in fact, it is the most complete popular description we remember to have seen; and if compiled entirely by Mr. Robinson from the books he has consulted reflects great credit on him, and we can but wish he had paid the same attention to the other parts of his treatise.

We think, if we remember rightly, that it was in one of the Arctic voyages, that of Sir John Ross, that the influence of atmospheric pressure on the rise of the tide was first observed, but the fact is well known now, and is always allowed for by surveyors when ascertaining the mean level of the sea. This subject is of considerable practical importance, for it is sometimes the only guide we possess by means of which we can reduce our soundings to the same depth as those obtained at previous epochs. For instance, if the datum-mark to which the soundings have been referred, has, in the course of time, disappeared, the surveyor's first work is to ascertain the height on his gauges of the mean level of the sea. This he does by obtaining day and night observations for five or six consecutive high and low waters, carefully registering the barometer at the same time. Then meaning the results and allowing for the atmospheric pressure, it is astonishing how closely they agree. The mean level having been found, it is very easy to reduce the soundings to the former datum. For instance, if the soundings are reduced to low water ordinary springs, and their rise at springs is 16 feet, it is evident that the soundings must be reduced to 8 feet below mean level of the sea, to enable them to be compared with those previously obtained.

In his paragraph (201) on tide-gauges, Mr. Robinson recommends a string from a float over a pulley. It must be either a chain or wire, as a string is far too subject to contraction and expansion from atmospheric changes.

Self-registering tide-gauges are, we are glad to say, becoming much more common than they were, and we trust to see them established at every important point in the United Kingdom.

The eleventh chapter, on Soundings, may do very well to enable a sub-lieutenant to answer some questions in the examination-room, but is useless in practice.

Few surveyors think it necessary to accurately protract on a chart the position of the objects they use for sounding transits (Art. 210). Often the back mark is too far off to appear at all on the sheet, and the farther off it is the better, provided the atmosphere is clear; for a front mark the first conspicuous object in the foreground is seized—a conspicuous tree, the chimney of a house, the angle of a hedge, a boat hauled up on the beach, &c. If the back object is sufficiently far off, the lines of soundings are practically parallel, and the same mark may be used for the whole survey. It is also requisite to cross the lines of soundings, to avoid any chance of error. The sounding on the chart depends (1) on the leadsman, (2) on the officer fixing and registering, (3) on the tidal register, and (4) on the reductions being correctly made. Now there may be a mistake in either one of these, and consequently it is advisable to always cross the lines of soundings as a check. We have found that as good a method of checking the correctness of the results as any, is by running along the contour lines, as defined by soundings obtained at right angles to those lines.

Another remark on soundings we must also take exception to, viz. that (Art. 213) it is usual to make a reduction of a couple of feet below low water in doubtful cases.

With respect to under-currents (Art. 221), Mr. Robinson appears not to be aware of the methods pursued by Sir G. Nares in the *Challenger*, and Capt. Wharton in the *Shearwater*.

Chapters twelve and thirteen, on Chronometers, and Meridian Distances, are principally derived from Admiral Shadwell's notes on the management of chronometers, and here we recommend the student to the original rather than the copy.

The method of calculating meridian distances expounded by Dr. Tiarks, and fully explained by Sir Chas. Shadwell, is invariably used by nautical surveyors, and the results thus obtained have hitherto closely approximated to the later determinations by means of the electric telegraph.

Chapter fourteen, on the method of Plotting a Survey, deals almost exclusively with the small plans required by the examination papers at the Naval College, and as it

instructs the student to plot *out* from the base line (which is never done in practice) cannot be recommended.

The method of plotting adopted in practice is to calculate out from the base line to the extreme points of the survey, or to the extreme points that will appear on any one sheet of paper, and then to plot *in*. Every practical draughtsman knows that it is far easier to say, "draw a straight line" than to do it, and that an infinite amount of trouble is saved by plotting in towards small triangles from large, as the errors of the plotter are then being constantly reduced, whereas in plotting *out* they are being continually enlarged. In fact we venture to say that no one is competent to write an article on plotting who has not been in the habit of projecting surveys for no one else can understand the extreme nicety required to make three lines from three stations to the same object coincide in one point.

It is possible that Mr. Robinson has compiled this work in hopes the Admiralty may order it to be accepted as the text-book on surveying at the College. We trust, however, that their Lordships may be better advised on the point. Already we have one book, ordered to be used, which contains a theory on winds, not by any means accepted by meteorologists, and this theory has at present to be learnt by all the younger naval officers. Now we have no objection to any one theorising on wind, or any other subject, but what we do object to is that a book containing such theories should be ordered to be the standard work at the colleges, simply because the gentleman who wrote it holds, and worthily holds, a prominent position there. We think that although theories should not be absolutely excluded from textbooks, they should deal principally with well-ascertained facts, leaving the student to develop for himself a theory from those facts.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

A PERUSAL of Dr. Romanes' article on Natural Selection and Natural Theology, in the *Contemporary Review* for October, 1882, suggests a few remarks upon one or two points, which may not be out of place.

One would quite agree with Dr. Romanes in "insisting on the essentially distinct character of natural science and natural theology as separate departments of human thought." True as that is, in a just sense, how does it follow that there "is no point of logical contact between" the two? Does this mean that because natural phenomena can be reduced to laws and sequences of cause and effect, no legitimate or rational inference can be made by the human mind to a *causa causarum?* It would seem so, and that it must be so to justify his very thorough-going conclusion: (1) That Darwin's theory explodes *particular design* (which he chooses to identify with special or independent creation); and (2) that it does not allow us rationally to introduce the conception of "an ultimate cause of a psychical kind pervading all nature," the theory having "no point of logical contact with the theory of design even in the larger sense." That is, a *raison d'être* in particular is proved to be absurd; behind all secondary causes, one such may possibly exist, but it is not to be legitimately thought of!

Or does he mean only that Darwin's theory need not, and legitimately should not, concern itself with philosophy and natural theology? Very well: then let the disciples practise what they preach, and imitate their revered master, who was content to maintain that species became what they are by descent with modification, instead of by independent creation, leaving untouched the question whether or not they were designed to be what they are. If there be "no logical contact" between Darwin's theory and the theory of design, then this renowned investigator preserved more logical consistency than some of his followers: if he refrained because of "the essentially distinct character of natural science and natural theology," and because of his determination to consider only the former, he was no less consistent.

But after all, such questions may be consistent enough, and moreover they are inevitable; and so it is not wonderful that they are raised—and not rarely prejudged—on the scientific about as freely as on the theological side.

Anyway Darwin did not prejudge the question of design, while declining to discuss it, as is done, for instance, by the *dictum* that if the species of animals and plants were slowly evolved, the evidence of design has been utterly and for ever destroyed. That has been affirmed over and over, formerly in the main by the theologians, but now, when these have seen what it comes to, mainly by the anti-theologians; by both, seemingly, under a misapprehension of the real character of the evidence for design.

Dr. Romanes' view is fairly presented in his denial that, under our present knowledge, "the facts of organic nature furnish *evidence* of design of a quality other or better than any of the facts of inorganic nature." "Or, otherwise stated, there is nothing in the theory of natural selection incompatible with the theory of theism; but neither does the former theory supply *evidence* of the latter. Now this is just what the older theory of special creation did; for it would be proof positive of intelligent design, if it could be shown that all species of plants and animals were created, that is, suddenly introduced into the complex conditions of their life; for it is quite inconceivable that any cause other than intelligence could be competent to adapt an organism to its environment *suddenly*."

Is the writer of this quite sure that any cause other than intelligence could be competent to adapt existing organisms to their environment *gradually?* How has the former presumption—the contrary of which was quite inconceivable—been done away with? For this presumption arose, and had its full force under the consideration of animals and plants produced by natural propagation; and the then irresistible inference of intelligent design was drawn directly from their adaptations in themselves and to their environment; whence it was concluded that the series of phenomena must have been instituted somehow and at some time or times (sudden creation is no doctrine of natural theology) under intelligence. How is this presumption negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, yet so as always to be in compatible relations to the environment? We do not see how or why the inference, which was so cogent, should under the new showing become at once irrelevant and out of all logical connection with the facts of the case, which *quoad design* are just what they were. *Suddenness*—if that must needs be entertained—is of course incompatible with the Darwinian view, and also with the facts as we understand them; but *gradualness* is in nowise incompatible with design. Under the conception of Nature as the outcome of Divine intelligence, questions of time and mode, of generality and particularity, are well nigh devoid of real significance.

But what may be contended for, and what is probably meant, is that natural selection is a rival hypothesis to design, that it accounts for all adaptations in the organic world upon known physical principles, and so renders the idea of design superfluous, as some would say; or, as it is better stated by Dr. Romanes, renders the evidence of design from these adaptations of no other or better value than that from anything else in Nature. So that the argument from teleology "must now take its stand upon the broader basis of the order of nature as a whole." This last, sensible natural theologians are prepared for. But the whole is made up of parts; and it is a whole in which the designed (if such there be) and the contingent can never be accurately discriminated, in which, indeed, from the very nature of the case, limitation is inconceivable. This need not be wondered at, since we are equally unable to discriminate the two in human

action. The evidence of design may be irresistible in cases where we cannot indicate its limits. We can only infer with greater or less probability, according to circumstances, and especially according to relation to ends. Better evidence than that of exquisite adaptation of means to ends is seldom, if ever, obtainable of human intention, and in the nature of the case it is the only kind of evidence which is scientifically available in regard to superhuman intention. Now if means and ends are predicable of inorganic nature at all, it is only by remote and indirect implication; while in organic nature the inference is direct and unavoidable. With what propriety, then, can it be affirmed that organic nature furnishes no other and no better evidence of underlying intelligence than inorganic nature? The evidence is certainly *other*, and to our thinking *better*.

To make the contrary supposition tenable, it must be shown that natural selection scientifically accounts for the adaptation; that the survival of only the very best adapted, out of the brood of more or less adapted to the environment at the time, gives sufficient scientific explanation of the adaptability or actual adaptation of the organism. Certainly this has not yet been done, and it seems incredible that it ever will be. That organisms have undergone changes as the Darwinian theory predicates, and that these changes have been picked out and led on by natural selection, seems to me most probable. That the action of the environment in some wholly unexplained way induces organisms to movement and change which would not otherwise occur, is also probable; but such change appears to be a response of the organisms to the physical surroundings and stimuli. And this most important factor in the result receives no explanation from the natural selection which operates upon it or co-operates with it. In other words, real causes have been assigned under which, *given the requisite changes*, the actual diversity and adaptations of plants and animals must or may have come to pass. But none have been assigned under which the organisms *must* have responded in the ways they do, or have responded at all, to the influences of the environment. Yet this is the very gist of the matter. The whole tenor of Darwin's writings and many explicit statements assure us that he completely recognised this distinction, which less exact minds overlook. If this distinction is valid, then the conclusion is at least premature which affirms "that the argument from teleology has been dislodged by the theory of natural selection," and its special value, as derived from adaptations in organic nature, utterly and for ever destroyed." ASA GRAY

Cambridge, U.S.

Intelligence in Animals

MR. ROMANES remarks in his book that there are few recorded instances of intelligence in bears; the following facts may therefore be worth recording:—In the Clifton Zoological Gardens there are two female Polar bears between two and a half and three years old, which came here quite young. One of these shows remarkable intelligence in cracking cocoa-nuts. A nut was thrown to-day into the tank; it sank a long way, and the bear waited quietly till after some time it rose a little out of her reach. She then made a current in the water with her paw, and thus brought it within reach. This habit has already been several times noticed in Polar bears. She then took it on shore, and tried to break it by leaning her weight on it with one paw. Failing in this, she took the nut between her fore-paws, raised herself on her hind-legs to her full height, and threw the nut forwards against the bars of the den, three or four feet off. She then again leant her weight on it, hoping she had cracked it; but failed again. She then repeated the process, this time successfully. The keeper told me she employed the same method to break the leg-bone of a horse. That this is the result of individual experience, and not of instinct, is clear from the fact that her companion has not learnt the trick of opening them thus, nor could this one do it when she first came. The method of throwing is precisely similar to that adopted by the Cebus monkey described by Mr. Romanes. J. G. GRENFELL

Clifton College, Clifton, Bristol, January 15

On a Relation existing between the Latent Heats, Specific Heats, and Relative Volumes of Volatile Bodies

As I do not find that the following relation between the latent heat of evaporation, the expansion undergone in changing into the gaseous state, and the specific heat of a volatile body has been previously pointed out; and as, if verified, it might be of some value in the determination of one or other of the above quantities I submit it, not however without considerable diffidence, to the readers of NATURE.

Briefly stated the relation stands thus—

The latent heat of gasification at constant pressure of any body, divided by the product of the relative volume of the gas and the specific heat of the body is approximately constant; or, if

λ = latent heat of gasification of any body,

v = relative volume of the gas; *i.e.* the vol. of the body on assuming the gaseous state compared with its vol. as a liquid,

s = specific heat of the body. Then

$$\frac{\lambda}{v \times s} = \text{const.}$$

The calculated value of this constant approximates to 0·8, as will be seen in the following table.

The letters λ, v, and s, heading columns 2, 3, and 4, have the same signification as above.

	λ.	v.	s.	$\frac{\lambda}{v \times s}$
Ether	91·11	228	·515	·775
Carbon disulphide	86·67	414	·235	·890
Wood spirit	263·7	651	·645	·628
Bromine	45·9	510	·107	·841
Oil of turpentine	68·73	204	·410	·837
Formic acid	169·0	548	·536	·575
Ethyl acetate	92·6	209	·527	·840
Methyl acetate	110·2	321	·507	·677
Butyric acid	114·67	271	·503	·841
Ethyl formate	105·3	241	·513	·851
Amyl alcohol	121·0	268	·587	·967
Acetone	129·7	339	·530	·736
Alcohol	208·0	456	·547	·833
Benzene	91·47	282	·395	·821
Chloroform	61·0	318	·232	·828
Perchloride of carbon.	47·0	263	·198	·902
Phosphorus trichloride	51.42	311	·209	·790
Methyl butyrate	87·33	273	·487	·657
Ethyl chloride	93·0	320	·427	·679
Ethyl iodide	46·87	317	·162	·914
Acetic acid	102·0	515	·503[1]	·393
Chloride of arsenic	46·5	324	·176	·813
Tetrachloride of tin	30·53	237	·148	·869
Water	537·0	1612	1·000	·333

It would appear then that the latent heat of a body may be considered as approximately proportional to the expansion of the body in vaporising and to its specific heat; and that the amount of heat required to convert a unit mass of the body at the boiling point from the liquid to the gaseous state, is equal to an amount of heat which would raise through one degree a quantity of the body in the liquid state which is approximately proportional to the expansion undergone by the liquid on evaporating.

It will be noticed that among the bodies instanced in the table there are some which appear to be very far indeed from according with the relationship in question. Notably acetic acid and water; of these, however, water presents so many peculiarities that perhaps it may be allowable to consider this as only adding one more to their number. In the case of acetic acid it is noteworthy that in plotting the curve of the latent heats of the group of acids of which acetic acid is a member, Favre and Silbermann found an irregularity arising from this body. It is, at any rate, possible that this irregularity may mean an error in the determination of the latent heat of this body.

Considering the difficulties which attend the accurate determination of latent heats, relative volumes, and specific heats of the several bodies, and that, of course, an error in any one of these will introduce inaccuracies into the constant, it may well be supposed that some, at least, of the variations noticeable in the results tabulated arise from inaccurate data. Further, there are in many cases two or more distinct determinations of these physical properties extant, of which one might be so selected in each case as to reduce the variations occurring in the constant to a minimum. F. TROUTON

Trinity College, Dublin

The Gresham Funds

IN an account of a meeting of the Common Council of the City of London, held last week, I read in the *Times* that the

[1] Varies with temperature of determination irregularly.

Gresham Committee reported that they had agreed with the Mercers' Company upon a scheme by which the open area of the Royal Exchange should be roofed over at a cost of 10,000*l*. Does this mean that the funds of the Gresham Estate are particularly flourishing just now, or that they are to be burdened with a new liability which will indefinitely postpone the time when there may be a surplus to be devoted to that advancement of science which Sir Thomas Gresham had in view in forming Gresham College? It is not long since some of the bonds, which represent money borrowed on the Gresham Estate for the building of the Royal Exchange, were advertised for repayment out of its surplus annual income, which afforded a hope that a good time for the scientific part of Sir Thomas Gresham's bequest might be drawing nigh. The public would be glad to know whether this hope is to be falsified or not. W. B.

January 20

Siwalik Carnivora

MAY I ask space to thank your correspondents for their answer to my previous inquiries concerning collections of Siwalik fossils in England, (many of which I have not yet had an opportunity of visiting,) and to add that I am now about undertaking the description of Siwalik Carnivora for the Indian Government? All remains of this order are very scarce, and in general fragmentary, and every specimen is, therefore, important. If any specimens exist in any provincial collections, I should be very glad of any information regarding them, and if possible of the opportunity of describing them in my forthcoming memoir. Any specimens sent to me, to the care of Dr. H. Woodward, F.R.S., British Museum (Natural History), Cromwell Road, S.W., will be thankfully received, and duly returned after comparison and description, if necessary. RICHARD LYDEKKER

The Lodge, Harpenden, Herts, January 17

Earthquakes

EARTHQUAKE phenomena are extremely rare in this highly favoured part of the world; but we had a very decided shake near the close of the year 1882. It occurred last night (Sunday, December 31) at about five minutes past ten o'clock, Halifax time, as nearly as can be determined at present. My observation was made at Lucyfield, ten miles north from the city of Halifax; the house stands on a rounded hill formed of unaltered drift, overlying slate rock, and at an elevation of about 350 feet above sea-level.

The air was perfectly still. There was a sudden rumble as of heavy waggons on a hard road at some little distance, then the sound became louder, I may say deafening, as of heavy loaded waggons running close to the wall; or of a heavy railway train running through a reverberating cutting; then the noise seemed overhead as if caused by rolling heavy furniture on the upper floor; there was a slight vibration of the building, as if something large and heavy had struck the roof, icicles fell from the eaves, fragments of plaster fell down behind the lathing of the walls, and there was a sound like a sudden gust of wind upon the windows and walls outside (there was no wind however). Suddenly noise and vibration ceased, and all was perfectly still. Passing outside, to look for some cause for these remarkable phenomena, nothing particular was noticeable. The country was covered with a thick white mantle of snow, the air was perfectly calm, there had been no rain drops, nor hail, but a faint flash of lightning (unaccompanied by thunder) occurred about a minute and a half or two minutes after cessation of the shock.

For two or three days prior to the shock, the temperature did not fluctuate much. The thermometer stood at zero centigrade (32° F.) at sunset on the 31st and has not varied much since; it was within a degree of the same at sunset of the previous day, but went down at night, rising again in the morning. During the day (31st) the sky was clear with some light fleecy clouds, wind northerly, but veered round to south-west about sunset, and the sky became overcast with clouds; later the clouds seemed to clear away, but the air became foggy, and was so at the time of the shock. (About Truro I am informed the sky was "clear and starry.") The air had been in a highly electric state during the afternoon and evening.

The earthquake shock lasted, as nearly as I can compute from recalling circumstances, something less than a minute, certainly more than half a minute, but probably not more than a whole one. I cannot indicate with any degree of certainty the direction of oscillation; so far as a retrospect of circumstances and sensations indicate, the apparent movement was from south-west to north-east.

Most persons in the city of Halifax to whom I have spoken to day observed the shock more or less distinctly, but it does not appear to have been nearly as violent in the city as in some other places. I ascertained from the conductor of the morning railway train that the shock was felt more or less severely all along the journey traversed by his train this morning, viz. from Truro to Halifax, a distance of sixty-one miles. At Shubenacadie, nearly thirty miles from my point of observation, flower-pots in the railway station house were toppled over on the window-sill and rolled upon the floor.

I have jotted down these particulars, thinking they may possibly prove of some interest if compared with the observations of others at different points. GEORGE LAWSON

Dalhousie College, Halifax, Nova Scotia, January 1

A SHOCK of earthquake was felt in this district on Tuesday, January 16, about 5 p.m. Comparatively few persons perceived it, but to those who did it was a striking phenomenon. The following report has been handed to me by a trustworthy observer:—

"About 5 p.m. on Tuesday, January 16, I was standing in a room, leaning against the foot of an iron bedstead, and facing a window, in front of which, on a table, was a cage containing one of the small African parrakeets known as love birds. The room was perfectly quiet, when this bird, which had settled itself for the night, surprised me by craning out its neck and flattening its plumage with every appearance of alarm, without any sound or movement on my part, or anything in the room which could possibly have frightened it. Immediately afterwards the iron bedstead I was leaning against, as well as the floor, trembled sufficiently to make me wonder what on earth was going on, especially as I heard no sound sufficient to account for it. The trembling ceased in a few seconds, and, while I was still wondering, returned in a greater degree than before, lasting this time about five seconds. The feeling I experienced was similar to that of standing on a bridge while a load was passing over. The second time I speedily came to the conclusion that it was caused by an earthquake." GEORGE F. BURDER

Clifton, January 20

I SHALL feel obliged if you will put on record in your columns that an earthquake was felt at Hastings by my sister and myself in separate rooms, on Tuesday morning last, the 16th inst., at 9½ minutes past 9 a.m. The undulatione were between E.S.E. and W.N.W., and lasted about 4 seconds.

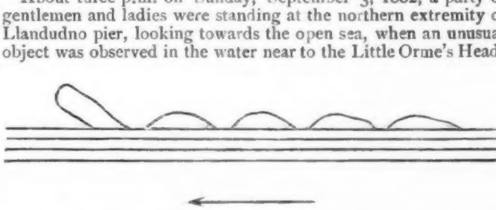
R. H. TIDDEMAN

H.M. Geological Survey, 28, Jermyn Street, S.W., Jan. 20

The Sea Serpent

BELIEVING it to be desirable that every well-authenticated observation indicating the existence of large sea serpents should be permanently registered, I send you the following particulars.

About three p.m. on Sunday, September 3, 1882, a party of gentlemen and ladies were standing at the northern extremity of Llandudno pier, looking towards the open sea, when an unusual object was observed in the water near to the Little Orme's Head,



travelling rapidly westwards towards the Great Orme. It appeared to be just outside the mouth of the bay, and would therefore be about a mile distant from the observers. It was watched for about two minutes, and in that interval it traversed about half the width of the bay, and then suddenly disappeared. The bay is two miles wide, and therefore the object, whatever it was, must have travelled at the rate of thirty miles an hour. It is estimated to have been fully as long as a large steamer, say 200 feet; the rapidity of its motion was particularly remarked as being greater than that of any ordinary vessel. The colour

appeared to be black, and the motion either corkscrew like or snake-like, with vertical undulations. Three of the observers have since made sketches from memory, quite independently of the impression left on their minds, and on comparing these sketches, which slightly varied, they have agreed to sanction the accompanying outline as representing as nearly as possible the object which they saw. The party consisted of W. Barfoot, J.P. of Leicester, F. J. Marlow, solicitor, of Manchester, Mrs. Marlow, and several others. They discard the theories of birds or porpoises as not accounting for this particular phenomenon.

F. T. MOTT

Birstal Hill, Leicester, January 16

A Novel Experiment in Complementary Colours

THE old maxim of an *adjacent gray* in order to give visibility to a complementary colour seems to hold its ground. Mr. Charles T. Whitmell puts it very clearly when he alludes to "the advantage of a reduction of brightness to a level comparable with that of the existing colour."

Mr. Whitmell will find, I think, that this brightness may be still further reduced below the level of the existing colour. This may be shown by one or two remarkable experiments with light admitted through a small needle hole the one-fiftieth of an inch in diameter, made through the bottom of a half ounce pill-box painted inside with lampblack. On placing a sheet of white paper on the table at night in a room lighted with ordinary gas, and looking through the small hole with one eye, *both eyes being open*, he will see on the paper a disc of a beautiful cobalt *blue* colour, evidently the complementary of the yellow light of the gas. On examining the sky in the same way in the morning, there will be seen, especially if the weather is dull and hazy, as it has been of late, a disc of a *primrose yellow* colour, the complementary of the blue sky, which, although invisible, is still making its impression on the sensitive retina. Later on in the day, between five and six o'clock, when the weather is murky, the disc has a well-marked *pink* colour, the atmosphere being evidently tinged with dark green. The several results I have witnessed from day to day for the last fortnight, and they have been verified by others to whom I have shown them. But when the sky is very blue and clear, there is seen, for obvious reasons, a blue disc only.

In the above experiments there is the curious anomaly of having one eye impressed with the exciting colour, the other with its complementary.

JOHN GORHAM

Bordyke Lodge, Tunbridge, January 20

The Projection of the Nasal Bones in Man and the Ape

IN my letter in the last number of NATURE (p. 266) the walls of the human nose were carelessly ascribed to the elevation of the *pre*-maxillary bones. This is not the case. It is only in the ape and lower animals that the ascending processes of the premaxillary assist in forming the external nose-case, or muzzle, *above* the nostrils. The frame-work of the nose in the lower types of the negro seems, therefore, in this respect, to differ more from the nose-case of the ape, than it does owing to any great development of the nasal bones.

I take this opportunity to mention that the woodcut of the embryo, which I referred to, appeared first in Quain's "Anatomy." Also, the quotation about the nasal bones of the orang, I have since found from my notes, to have been derived from Prof. Mivart's "Man and the Ape."

January 22

J. PARK HARRISON

HOVERING OF BIRDS

THIS problem, to account for the phenomenon of the motionless hovering of hawks and other birds in mid-air, was the subject of correspondence in NATURE, vol. viii. pp. 86, 324, 362; vol. ix. p. 5; vol. x. pp. 147, 262: vol. xi. p. 364. The only plausible explanation advanced (by Joseph Le Conte, vol. ix. p. 5, and previously by the present writer, vol. viii. p. 362) was that the birds take advantage of slant upward currents of wind sufficiently strong to neutralise the force of gravity. But the arguments brought forward in support of this explanation were perhaps not quite conclusive, for lack of a sufficient series of observations.

During the past six years I have noted such instances as I have chanced to witness in the course of a wandering occupation, and now offer the results as a further contribution towards the solution of the question.

I may state at once that in every case where I have seen a bird hovering, the following three conditions have obtained:—

(1) There was a fresh wind blowing.
(2) The bird was facing the wind.
(3) Beneath the bird there was a steep slope of ground facing the wind.

The particular localities in which I have observed the phenomenon are the following:—

(1) 1877, *September* 17.—Driving from Aberayron to Llanrhystyd (Cardiganshire). Wind W.N.W., moderate. Cliffs facing N.W. Gulls under cliff top, below road, in poise. Hawk under hill top, above road, in perfect poise.

(2) 1877, *October* 13.—Approaching Llantrisant town (Glamorganshire) from Llantrisant Junction. Wind S.W., moderate. Hawk over S.W. slope, barely poising, partly fluttering, tail plainly brushed up.

(3) 1877, *October* 14.—Llantrisant (Glamorganshire). Wind S.S.W. Rooks upborne, above S.S.W. slopes of hill with entrenched fort (Caerau).

(4) 1877, *October* 20.—Cliff facing S. between Longland and Caswell Bays, Gower (Glamorganshire). Gull and crows upborne. Wind moderate, S.S.W.

(5) 1877, *October* 21.—Cefn Bryn, Gower (Glamorganshire), facing S.S.W., climbing from Reynoldston. Rooks upborne. Wind strong, S. by W.

(6) 1879, *October* 17.—On road from Llantrisant to Pontypridd (Glamorganshire). Wind W. Rooks upborne over slopes facing W.

(7). *October* 28.—Killay, near Swansea (Glamorganshire). Hawk poised above hill-side facing N.E., to the west of Killay railway station. Wind N.E. I was almost under the bird, and could see the conduct of wings and tail suiting the ripples of wind.

(8) 1879, *November* 5.—Near Merthyr Tydfil (Glamorganshire). Hawk poised over N. slope of hill above (to S. of) tunnel on Merthyr-Abernant Railway. Wind N.

(9) 1880, *March* 13.—Near Penally (Pembrokeshire). Sea-gulls, rooks, and jackdaws upborne and floating with wings outstretched all along cliff line facing S., between Penally and Lydstep. Wind S., full on cliff from the sea. Gulls up to 200 feet above cliff edge. At greater height and inland, they were flapping. Different behaviour of rooks over inland northern slope.

Further on, over caves at north end of Lydstep Sands, hawk poised for 1 min. and 1½ min. at a time, just over cliff line, in teeth of wind off sea.

(10) 1880, *March* 17.—Near Cardiff (Glamorganshire). Hawk poised about 10 or 12 feet above railway embankment facing E.N.E. (20 or 25 feet high) of Llandaff and Penarth line, near Ely Station. Wind E.

(11) 1880, *March* 27.—Gulls uplifted over E. scarp of Beachy Head Down (Sussex). Wind E.N.E.

(Same day).—Over N.E. slope of Lighthouse Down. Bevy of eight gulls, all in perfect poise, immediately over edge of cliff.

(12) 1880, *August* 8.—Wells next-the-sea (Norfolk). Wind N.W. Hawks poising over W. slope of sea-wall, and over N.W. slope of sand-hills (projecting from the main line of dunes that runs east and west), and trying unsuccessfully over railway embankment which runs N.W. and S.E.

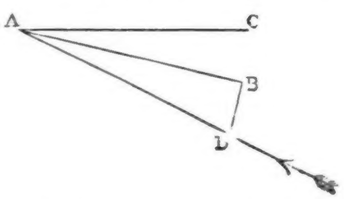
In several of the instances here recorded I was near enough to see that the bird was delicately adapting the slope and spread of its wings to the momentarily varying inclination and force of the wind. Among the sand-hillocks near Wells, on the Norfolk coast, I succeeded in approaching, under cover of ridges and long grass, within about ten yards of a hovering hawk, and saw the

posture of the bird very well for a few seconds, till he became aware of my presence and dashed away. I was much struck by those instances in which the obstacle that caused the upward slant of the wind was only a sea-wall or a railway embankment, and especially by the critical case (No. 12) where the bird was evidently baffled because the wind lay along the embankment, not against it, and therefore gave no upward current.

My list includes four cases (3, 4, 5, 6) of rooks and gulls "up-borne" on outspread wings, under conditions similar to those present in hovering—cases that could not be explained by any theory of *vis viva*, but clearly involved an external mechanical force, which could only be that of the wind, sustaining and uplifting the birds. The close relation between the "up-borne" and the "hovering" action was evident in case (9) where the gulls, &c., were up-borne and sailing, while the hawk was poised and motionless.

These observations, as far as they go, appear to indicate plainly the law which governs the phenomenon in question. I think they strongly confirm the theory already advanced, that the bird in hovering is upheld by a slant upward current of air. A strong wind pressing against a slope of ground is necessarily thrown into a slant upward current, "as slopes a wild brook o'er a hidden stone." There may be a downward eddy if the slope is precipitous, as one may often feel at the foot of a high wall, but the main stream of the air for some considerable height above the slope is forced to take an upward slant, with increased velocity, in order to surmount the obstacle in its path.

Given such a slant upward current, it is easy to see



that a bird, with the exquisite muscular sense that every act of flight demands and denotes, might so adapt the balance of its body and the slope of its wing-surface to the wind as to remain motionless in relation to the earth. The slope of the wing-surface should divide the angle between the horizontal and the direction of the slant wind-current in such proportion that, if the air were at rest, the bird, under the action of gravity, would float forwards, downwards, on outspread wings, with exactly the same velocity as that of the wind (in which it remains motionless) and in exactly the opposite direction. The mechanical conditions are identical in the two cases, whether we consider the air at rest and the bird floating through it, or the bird at rest and the wind rushing under it. In either case the bird has reached, and maintains, its maximum velocity, due to gravity, compatible with the resistance of the air, which resistance is the same in both cases.

I have heard it objected that the mechanical conditions are not the same in these two cases, because in the one case the bird has momentum, in the other not. Need it be said that momentum is a purely relative possession, just as velocity is? In each case the bird has the same velocity, and therefore the same momentum, relative to the air. The mechanics of the situation, as between bird and air, are not affected by the possession or loss of velocity (and with it momentum) relative to the earth.

Perhaps the feasibility of the thing may be best shown by a simple diagram. Let A B represent the slope of the bird's wing (viewed from the right side), dividing the angle between the horizontal, A C, and the direction of the wind, D A. Draw B D at right angles to A B, and take A D to represent the force of the wind. Then D B and B A will represent the force of the wind resolved perpendicular and parallel to the slope of the wing A B. The resolved part, B A, meeting only the resistance of the bird's head and shoulders and front edge of the wings, tends not strongly to push the bird in the direction, B A, that is, backwards and a little upwards. But the resolved part, D B, which meets the full area of the outspread wings and tail, tends powerfully to push the bird in the direction D B, that is, upwards and a little forwards. Then all that is required to keep the bird at rest is that the effect of the forward force exerted by D B should balance the effect of the backward force exerted by B A (both being resolved vertically and horizontally), and that the great upward force exerted by D B, together with the small upward force exerted by B A should exactly neutralise the downward force of gravity.

The only difficulty in the way of the slant-upward-current theory lies in the statement of the Duke of Argyll (NATURE, vol. x. p. 262) that "a hundred times" he has seen birds hovering "when by no possibility could any upward deflection of the wind have arisen from the configuration of the ground." My own observations testify so consistently in favour of slant upward currents that I feel justified in asking for more precise information concerning the instances alluded to by the Duke of Argyll, before relinquishing the theory which I hold. Wherever I have seen a hawk trying to remain in one position over a plain or slightly undulating ground, the feat has only been accomplished by continued vibration of the wings.

The problem of the "soaring" of birds introduces other conditions, which require separate consideration, though I believe it will be found that the two phenomena of "soaring" and "hovering" depend upon essentially similar causes.

(By the bye, does not the provincial name of one of the hawks, the "Windhover" record the constantly observed dependence of the act of hovering on the wind?)

HUBERT AIRY

THE LATE EDWARD B. TAWNEY

BY the death of this young naturalist English geology has lost one of its most enthusiastic and cultivated students. Hardly beyond the threshold of his career, he had already gained for himself a notable place among the geologists of this country, and his friends augured for him a future of distinction and usefulness. But in the fulness of his promise and in the midst of his work he has been struck down so suddenly that few of his friends knew he had been ailing until they were shocked and saddened by the news of his death.

Born in 1841, he was the third child of the Rev. Richard Tawney, Vicar of Willoughby, Warwickshire, who had gained a distinguished place at Rugby, and had been a Fellow of Magdalen College. On the death of his father, young Tawney was placed under the care of his guardian, Dr. Bernard of Clifton, and received his early education there. During these years he seems to have acquired a bent towards natural science mainly through the influence of Dr. Bernard and Dr. Fox of Brislington. He was eventually enabled to gratify this inclination by attending the courses of instruction at the Royal School of Mines, Jermyn Street, from 1860-63, where he greatly distinguished himself. He gained a Royal Scholarship, Duke of Cornwall's Scholarship, the De la Beche Medal for Mining, and the Edward Forbes Medal for Natural Science, and took Associate's diplomas in the Mining and Geological divisions.

With the training in scientific methods thus obtained, he soon betook himself to original research, gaining experience by excursions at home and by travel abroad. In 1872 he accepted the offer of Assistant Curator of the

Bristol Museum. With characteristic energy he at once set to work, re-tableting, re-arranging, and naming the geological collection, taking care to have gaps in the series filled up, and making the museum really serviceable for purposes of instruction. Six years later, in the early part of 1878, he received the appointment of Assistant Curator of the Woodwardian Museum, Cambridge. He soon made his mark there, as was acknowledged in the following year by the bestowal upon him of the honorary M.A. degree. His indefatigable industry and wide range of acquirements so peculiarly fitted him for this position, that his death must for some time to come be an almost irreparable loss to the University.

Looking over his published papers one cannot but be struck with his versatility. At one time we find him discussing the Rhoetic beds of South Wales, at another dealing with that vexed question of Alpine geology—the position of *Terebratula diphya*. From Devonian fossils he passes to the description of new species of Oolitic gasteropods, or to the Cretaceous Aporrhaidæ, or to Palæozoic star-fishes. He could enter minutely into the stratigraphy of the Isle of Wight Tertiary strata, and with not less energy and clearness of insight described the microscopic structure of the crystalline rocks of Wales. Well versed in the Continental languages, he kept himself abreast of the foreign progress of his favourite science. Nor were his tastes wholly scientific. He delighted in Piers Ploughman and the Niebelungenlied. What he might have done who may guess? That with his feebleness of constitution he should have been able to accomplish so much, shows how ardent was his love of nature and how indomitable his spirit of inquiry. His devotion to truth and abhorrence of everything savouring of insincerity or sham led him to speak out freely and uncompromisingly. But no one could mistake the honesty of his purpose. A. G.

REMARKS ON AND OBSERVATIONS OF THE METEORIC AURORAL PHENOMENON OF NOVEMBER 17, 1882

THE interesting meteoric phenomenon seen in England during the aurora of November 17 last, has induced me to endeavour to find the true path of that object. Though I have spent much time in applying the method given by Prof. E. Heis in his "Periodischen Sternschnuppen," I have got no farther than the point to which Mr. H. D. Taylor has brought us, the observations being in no way capable of combinating. In fact, when seeking the lines of intersection, formed by the different planes of the great circles, wherein the apparent path was seen, with the mean horizon (say the plane of a common map), these lines have but little tendency to converge to the same point. Therefore the method of Mr. Taylor seems to me the most convenient. When the object has followed a straight line, all the places where it was seen passing just before the moon, must lie in a plane containing the true path and the moon. This plane must cut the plane of the map in a straight line. Now the four places where observers saw the meteor before the moon's disc are:—Woodbridge, near Ipswich, Lincoln's Inn Fields (London), Windsor, and Ramsbury, near Hungerford, fulfilling, by no means, the above-mentioned condition. Nevertheless the most probable direction of this line seems to be that accepted by Mr. Taylor, N.E. by E.-S.W. by W. (astronomical), because this is the general direction of the lines of section, given by the great circles, mentioned above. Here it is to be remarked that when the meteor was seen from S.E. to S.W. (as in the case at York), but at some height (here 10°) above the horizon, the intersections of the apparent path with the horizon may lie near E. and W. (here, according to the observation of the meteor passing 6° below the moon, at 12° south of E.). We give here bearings as seen from the different places, taken directly from the communications, or deduced indirectly from them:—

Street (3° south of Leeds)	S.E.-S.W.
Clifton (Bristol)	E. 18° N.-W. 18° S.
Greenwich	E.N.E.- (?)
Guildown (p. 149)	E.-W. (nearly).
Bedford	S.E.-S.W.
Clevedon (p. 100)	N. 70° E. (?)-S. 70° W.
Cambridge	E.-S.S.W.
York (H. D. Taylor)	E. 12° S.-W. 12° N.
Woodbridge	E. 10° N.-W. 10° S.
Windsor	E.-W.
Coopers Hill	?-S.W.
Ramsbury	?
Lincoln's Inn Fields	E.-W.

Now we can add to these English observations[1] two others made in the Netherlands.

1. Prof T. A. C. Oudemans gives in the *Utrecht Newspaper* (No. 318) the following (translated) description:—"At 6h. 23m. (6h. 2½m. Greenw. T.) a feather-like appearance, resembling in the beginning a brilliant comet, formed suddenly in the eastern part of the heavens, the end being just before Aldebaran. Within two minutes this feather had prolonged itself above Saturn, through the Pegasus quadrate, and south of the three Eagle-stars, the east or following end shortening, while the other or preceding end advanced. . . . When this arch had obtained the length of 90° (which lasted but a few seconds) a separation was made in the middle of its length, where the arch had a breadth of about 3°. This separation had a length of about 10° and a breadth of ½°, and was pointed at the ends. At 6h. 25m. this arch disappeared wholly in the west." Prof. Oudemans says further that the great circle of the apparent path intersected the equator at 110° and 290° of right ascension. This gives me, combined with the position of Aldebaran, a direction in the horizon of E. 20° N.-W. 20° S.

2. Mr. P. Zeeman observed the same phenomenon at at Zonnemaire, near Zierikzee (51° 42′ lat. and 57′ W. Amsterdam). He wrote me the following on November 19 and 24:—"About 6h. 20m. (I saw) a magnificent, splendid white arch, beginning a little north of east, and prolonging itself to south-west, but in the meantime shortening at the east end and disappearing in a very short time." Mr. Zeeman declares in his second letter that this arch went through Aldebaran, and through α Pegasi. This gives me a horizontal bearing of E. 20° N.-W. 20° S., as the observations of Prof. Oudemans gives also.

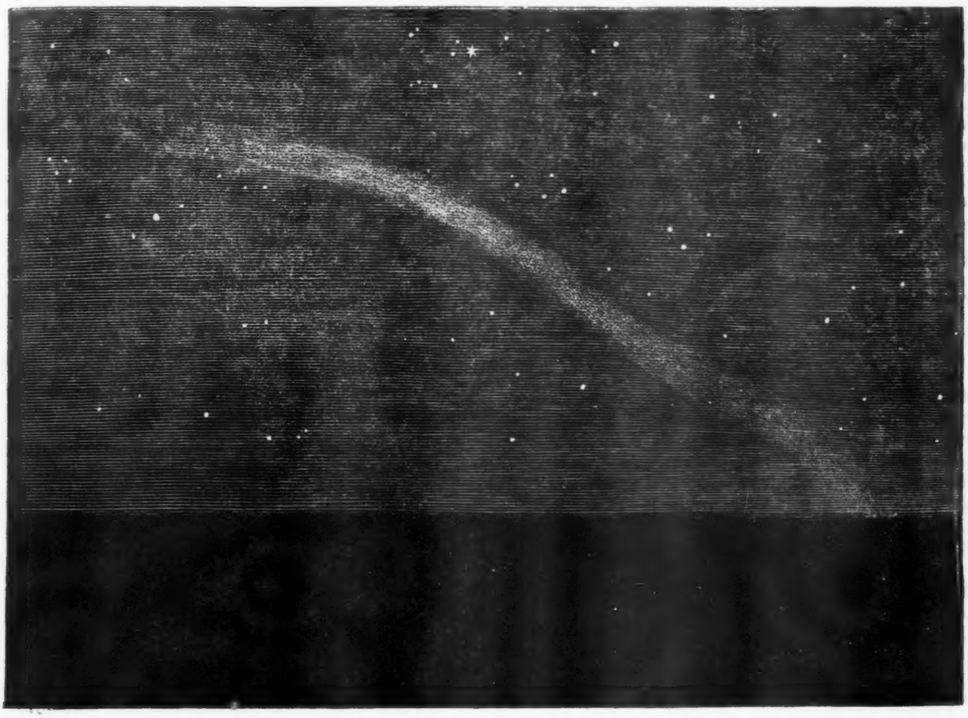
Thus we find these two Dutch observations (unhappily the sky in Groningen had just, at 6h. 1m. Greenw. T., got cloudy, the aurora being very splendid before) supplement and confirm the greater part of the English observations. Only the phenomenon seems to have been of greater apparent size, and therefore nearer to the observer. The separation by an obscure streak seems not to have been visible in England, perhaps from the change of its relative position.

The conclusion to which we come after all, regretting earnestly the want of French observations, is that we have here probably a meteoric object, moving, according to the calculations of Mr. H. D. Taylor (vol. xxvii. p. 140), with great velocity through the upper strata of the atmosphere and at the same time of auroral character, as the spectrum observation of Mr. Rand Capron (vol. xxvii. p. 84), makes out beyond any doubt. The separation and the feather-like forms, observed at Utrecht, make it probable that it was a mass of meteoric dust, passing through our atmosphere like an accumulation of little shooting stars. In this way the phenomenon of November 17 brings a confirmation of my own theory of auroræ, proposed by me in the "Appendice alle Mémorie della Società degli Spettroscopisti Italiani," 1878, vol. ii., and received with sympathy

[1] Will Mr. W. M. Flinders Petrie be so kind as to tell us where we can find the Swedish observation mentioned by him, vol. xxvii. p. 140?

by many of the German and Dutch astronomers; but as it seems little known in England, though referred to by Mr. Rand Capron on p. 64 of his beautiful work, "Auroræ." In this theory most of the properties of auroræ are deduced from cosmic dust entering into the atmosphere of the earth. I take the liberty to direct attention to the unexpected argument, that the brilliant object of November 17, 1882, has brought forth in favour of my "Théorie Cosmique," to which I had already the opportunity to refer in this Journal in my article "On Dust, Fogs, and Clouds" (vol. xxiii. p. 195).

Furthermore, I think that this object is not the only example of such a phenomenon. On November 2, 1871, there was seen in Groningen and several places of Germany a strange, brilliant arch, striped parallel to its well defined sides and changing its curve during its two hours of existence. The beginning of the phenomenon (of which I gave a description in the Dutch journal *Isis*) was seen by a student, Mr. Gratama, like an elliptic patch of light round the Pleiades. Dr. Vogel, who observed the same arch at Bothkamp, determined its auroral character by the spectrum. Otherwise it resembled very much the bright spur of a gigantic meteor or fireball. Also it disappeared slowly, beginning at the east end, as the illustration shows. A faint aurora, with dark segment, was visible in the north. The height of this arch was calculated by me approximately at 127 kilometres or 79 miles. I think that the only difference between these two feather-like phenomena of November 2, 1871, and of November 17, 1882, consisted in the different apparent velocity and in the greater mass of meteoric dust, forming in the case of November 17, 1882, but a short, and in that of November 2, 1871, a very long train of incandescent matter. It must be remembered here



Auroral Arch, observed November 2, 1871, at Groningen (Netherlands).

that the tails of great fireballs remain visible for half an hour or more (see *e.g.* the article of Mr. Branfill, vol. xxvii. p. 149). In NATURE, vol. xii. p. 330, is to be found a description of similar arches, seen at Fremantle in Australia by Mr. Lefroy, in presence of the moon, which was obscured by one of them.

This leads us to a question, touched by Mr. Backhouse, NATURE, vol. xxvii. p. 198, that of the halos seen in Siberia (by Von Wrangel, I believe), when an auroral beam was in front of the moon. I watched in vain if such an event should perhaps occur November 17 last, but Mr. Zeeman, whom I have cited above, seems to have been so happy as to have seen a white and bright auroral cloud floating over the moon's disc at 5h. 47 (local time), giving the common interference phenomena. It is unnecessary to remark, that these phenomena can be formed by all kinds of dust, formed of nearly equal particles, and that they in no way require ice-particles. On my inquiry why the observer could decide that it was not a common cloud, he brought forward the following arguments:—(1) Its great brightness; (2) its transparency to the starlight; (3) its very great velocity, unusual in common clouds.

Returning to the meteoric phenomena, visible simultaneously with auroræ, it seems that such phenomena were seen during the marvellous aurora of January 7, 1831, described in *Poggendorff's Annalen* of the same year. We read (p. 440) that Bergrath Senff, in Colberg, at 6.30 o'clock, saw above the west horizon a bright yellow streak, rising upward with a common cloud-velocity, passing at 30° N. Zen. D., and forming an arch from W. to E., beginning to disappear from the west end, almost at the same moment that it reached the east horizon. At p. 458 we see that Prof. Rudberg, at Upsala,

December 7, 1830, saw a very bright patch of double the dimensions of the moon's disc, moving with great velocity behind the common auroral beams. Further, Prof. Bischoff, in Burgbrohl (p. 461), observed, on the occasion of the aurora of January 7, 1831, a moving cloud as bright as the milky way, from E. to W., in five minutes. Prof. Moll saw, in Utrecht a similar object, rising from N.E., through the Pleiades, to S.E. (S.W. ?). Similar observations are to be found during the same aurora, p. 471 (one advancing arch), p. 472 (four similar arches, and a dark streak).

In several articles on the aurora of November 17, 1882, the height of auroræ is spoken of. Mr. W. M. F. P. (p. 173) says that the strange object observed is *physically impossible* to auroral nature, because of its height of about 170 miles. It was already observed by Mr. Backhouse that auroræ are often observed at very great heights. The same is also the case with shooting stars. I take the liberty to refer once again to an article of mine in this journal entitled "The Height of the Aurora," where I refer to the beautiful determinations by Prof. Heis and Dr. Flögel, published in the *Zeitschrift der Oesterr. Gesellsch. f. Meteor.*, vii. p. 73. The heights were found from 10 to 100 geogr. miles (46 to 461 Engl. miles). Dr. Sophus Tromholt found, besides apparent low heights of some auroræ in Norway, the height of that of March 17, 1880, to be 17 geogr. miles ("Wochenschrift redigirt," von Dr. H. J. Klein, 1880, p. 172). Prof. Galle of Breslau calculated by his method, described in the *Zeitschr. f. Met.*, vii. p. 73, and in the *Astr. Nachrichten*, Bd. 79, No. 1882, 40 to 60 geogr. miles, and I found for the great aurora of May 13, 1862, 59 geogr. miles.

H. J. H. GRONEMAN

Groningen (Netherlands), January 14

NOTES

THE Council of the Institution of Civil Engineers have arranged for the delivery at the Institution of a series of six lectures, on the Applications of Electricity, on the following Thursday evenings, at 8 o'clock :—February 15—The Progress of Telegraphy, by Mr. W. H. Preece, F.R.S., M.Inst. C.E. March 1—Telephones, by Sir Frederick Bramwell, F.R.S., V.P.Inst., C.E. March 15—The Electrical Transmission and Storage of Power, by Dr. C. William Siemens, F.R.S., M.Inst. C.E. April 5—Some Points in Electric Lighting, by Dr. J. Hopkinson, F.R.S., M.Inst. C.E. April 19—Electricity applied to Explosive Purposes, by Prof. F. A. Abel, C.B., F.R.S., Hon. M.Inst. C.E. May 3—Electrical Units of Measurement, by Sir W. Thomson, F.R.S., M.Inst. C.E. This is an excellent step which the enterprising Institution has taken, and we are sure will be productive of good both to science and to engineering.

MR. ERNEST H. GLAISHER, B.A., Trinity College, Cambridge, has been appointed Curator of the British Guiana Museum, George Town, Demerara.

MR. W. H. WHITE, one of the Chief Constructors to the Navy, has resigned his position to take up a managerial appointment in the firm of Sir Joseph Whitworth.

AN interesting boring through the chalk is now about to be resumed at Southampton. At the last meeting of the British Association a paper by Mr. T. W. Shore and Mr. E. Westlake on the Artesian well on Southampton Common was read in the Geological Section. The Town Council has now accepted a tender for continuing the boring which was abandoned in 1851, after a depth of 1317 feet had been reached. The boring was then passing through the lower chalk or chalk marl, and we believe it is now intended to continue it to the Lower Greensand. The well at the bottom of which the boring commences is 563 feet deep, and this was reopened last week, after having been closed for thirty-two years. Some observations on the temperature of the water were at once made by Mr. T. W. Shore and Mr. J. Blount Thomas, of Southampton, for the Underground Temperature Committee of the British Association. By means of a heavy elongated sinking weight and a registering windlass, a thermometer was passed down the bore shaft to a depth of 1210 feet, when it was stopped by chalk mud. An outer case which was attached to the sinking weight was much scratched in passing through the Upper Chalk. A temperature of 71°·9 F. was registered at the bottom, that of the outer air being 49° F.

THE City of Neuchâtel celebrated in the beginning of the present month the fiftieth anniversary of the foundation of its Natural History Society. The leader among its founders, who first met for the purpose on December 6, 1832, was Louis Agassiz.

THE biennial Hunterian oration will be delivered on Wednesday, February 14, at three o'clock, by the President of the College of Surgeons, Mr. Spencer Wells, in the theatre of that institution. The biennial festival will be given in the library the same evening, to which the president and vice-presidents have, as usual, invited several distinguished visitors.

THE Pontifical Academy of the Nuovi Lincei have appointed a Committee to take steps for the erection of a monument in Rome to the late eminent astronomer, Father Secchi. The monument will be of a meteorological character. The sculptor Prinzi has already made a model which combines convenience for arranging the meteorological apparatus with features recalling the work of Father Secchi. The statue of the astronomer crowns the monument, and among other emblematical figures will be one of Meteorology holding in one hand a gigantic barometer, which can be seen from a great distance, and another of Physics holding up to view an equally large thermometer.

THE rumour that the fragments of the unfortunate Mr. Powell's balloon have been found in the Sierra del Pedroso, in the far south of Spain, is too vague and incredible to deserve much attention.

AT the meeting of the Essex Field Club, to be held on Saturday evening next, January 27, the attention of the members and the public generally will be directed to the Bill about to be introduced into Parliament for the construction of a line of railway from Chingford to High Beach. In January, 1881, the Club, in conjunction with other Natural History Societies in and around London, strongly protested against any portion of Epping Forest being occupied by a Railway or other Company, to the prejudice of the provisions of the Epping Forest Act, and certainly no sufficient arguments or expressions of public opinion have since been brought forward in favour of the scheme. It is believed that the proposed line is quite unnecessary, as no part of the forest is more than two miles from a railway station, and moreover a railway and its concomitants could not fail to destroy the chief interest and charm of the district—its seclusion and naturalness; qualities of inestimable value so near a large city.

THE following papers are set down for reading at the meetings of the Society of Arts during the part of the Session after Christmas :—At the Ordinary Meetings—W. K. Burton, The Sanitary Inspection of Houses; General Rundall, The Suez Canal; Prof. Thorold Rogers, M.P., Ensilage in the United States; Sir Frederick Bramwell, F.R.S., Some Points in the Practice of the American Patent Office; J. H. Evans, The Modern Lathe; A. J. Hipkins, The History of the Pianoforte; Prof. George Forbes, The Electrical Transmission of Power;

D. Pidgeon, Recent Improvements in Agricultural Machinery; Wilfred Cripps, F.S.A., English and Foreign Silver Work, with some Remarks on Hall-marking. In the Foreign and Colonial Section—Edmond O'Donovan, Life among the Turkoman Nomads; Rev. J. Peill, "Social Conditions and Prospects in Madagascar; Robert W. Felkin, Egypt: Present and to Come; W. Delisle Hay, Social and Commercial Aspects of New Zealand. In the Applied Chemistry and Physics Section—C. F. Cross, F.C.S., Technical Aspects of Lignification; Walter G. McMillan, F.C.S., Chemical Means for Preventing or Extinguishing Fires; W. N. Hartley, F.R.S.E., Self-purification of River Waters; R. W. Atkinson, B.Sc., The Formation of Diastase from Grain by Moulds; James J. Dobbie, D.Sc., and John Hutchinson, On the Application of Electrolysis to Bleaching and Printing. In the Indian Section—Charles H. Lepper, Overland Commercial Communication between India and China, *viâ* Assam; W. S. Seton-Karr, Agriculture in Lower Bengal, with some Notice of Tenant Right, &c.; J. M. Maclean, Private Enterprise in India; C. Purdon Clarke, Some Notes on the Domestic Architecture of India.

WE have received from Egypt a publication of some interest in the shape of the *Bulletin* of the Chemical Laboratory at Cairo, directed by M. Albert Ismalun. The laboratory is under the Department of Public Works, and judging from the report in the *Bulletin* is doing a fair amount of useful work. The laboratory has been recently much improved, and attached is a museum of specimens in geology, palæontology, and zoology.

A CONSIDERABLE number of names has been added to the list of those who are unfavourable to the meeting of the British Association in Canada in 1884. The request of the protesters to the Council seems to us quite reasonable,—"that it is highly desirable that you should take some further steps in order to ascertain the general feeling of the members of the Association upon the subject, before allowing our kind and liberal friends in Canada to incur any further trouble or expense." There are 141 names appended to the circular, and while some of them are well known, still we note the absence of some of the leading representatives of English science.

THE proceedings at the meeting of the Association for the Improvement of Geometrical Teaching were not carried out quite on the lines laid down in a recent number of NATURE (vol. xxvii. p. 247). In consequence of a delay in the delivery of the copies of "the Elements of Plane Geometry," the President was obliged to defer the moving of his resolution till the next meeting, which, it is hoped, will be held about Easter next. The members were also informed that Mr. Levett had, in answer to an appeal made to him, consented to retain office as Hon. Secretary for the present year. Mr. E. B. Sargant, Trinity College, Cambridge, was elected to act as joint secretary with Mr. Levett. The following members were elected: Miss Burstall, Professors G. C. Foster, F.R.S., W. H. H. Hudson, H. Lamb, and G. M. Minchin, Rev. A. Jamson Smith, and Messrs. G. Griffith, E. B. Sargant, Charles Smith, F. Turner, and H. H. Turner.

DR. PEVERATI, director of the Meteorological Observatory of Cassine, states that an earthquake shock was felt there on January 16, at 7.42 a.m. (Roman time). The shock was undulating, preceded by a rumbling noise in the direction W.E., and lasted three-quarters of a second. The accompanying noise is compared to that of a very heavy body in motion in contact with another body at rest. The shock is classified as No. 3 in the scale of intensity proposed by Dr. Forel. A similar shock was felt the same day at Demonte (Cuneo) at 5.25 a.m., moving in a west-south direction. On the night of the 14-15th, several shocks were felt at Terranova and Pollino in Basilicata. Twenty-two shocks of earthquake were felt on January 16, at Centi, in the province of Murcia, Spain. Several houses were destroyed, but the inmates escaped unhurt. There was no loss of life.

IT is announced from Mexico, January 23, that a new comet near Jupiter has been discovered at the Puebla Observatory.

A NEW electrical paper *Electricity*, has issued its first number at Buda-Pesth. It is written in the Magyar language. The first paper of this description ever published was called *Les Archives de l'Electricité*, and was published by M. de la Rive at Geneva in 1840, and the first issued in England was edited by the late Mr. Walker in 1843, under the title, *Electrical Magazine*. None of these papers lasted for more than three or four years.

THE Algerian Government is preparing an expedition for next spring, in order to protect effectually the southern part of the province of Oran against incursions of the surrounding independent tribes. The results of this expedition are not without interest for English journals, many of which are printed on paper made from alfa, a plant cultivated in those remote regions, and manufactured in England. A curious fact is, that no French paper-maker ever attempts to manufacture alfa for inland consumption.

THE oases of the Beni-mzab Confederacy to the south of Algeria, have been annexed to the French Algerian possessions, and a military expedition has established a regular administration in the country. The Algerian section of the French Alpine Club is organising a scientific expedition which will leave shortly in order to take advantage of a favourable season for travelling. Any one wishing to take part in this excursion should communicate with M. Durando, president of the Algerian section. The newly-annexed oases are seven in number, with a population estimated at 40,000, with about 200,000 palm-trees under cultivation. The ruins of several large towns have been covered by sand.

TWO of the most important scientific expeditions which attempted to get into the Siberian seas last year were those in the *Dijmphna*, with Lieut. Hovgaard bent on reaching the North Pole, and the Dutch Meteorological Expedition in the *Varna* bound for Port Dickson. These two vessels succeeded in forcing the ice in the Waigatz Straits in September last, and perhaps the *Dijmphna* would then have got through the Kara Sea, had she not, by mistaking certain signals, been led to leave the open "lead" in which she was, and gone to the assistance of the steamer *Louise*, beset by the ice. She was caught in the pack, as the *Varna* had previously been, and was frozen in on September 17. The last report which we possess from these vessels, is dated September 22, and was brought to Europe by Capt. Dallmann of the *Louise*. Since that date no news whatever has come to hand from the vessels, and the statements which have appeared in the Russian press relating to the discovery by Samoyedes of a wreck, supposed to have been that of the *Dijmphna*, south of Waigatz Island, have been proved to refer to an old Russian whaler, stranded there some years ago. Although the expedition, if it had met with any mishap, would undoubtedly have found its way to the mouth of the Petchora, of which we should have had information before now, it has been decided by the Danish Government to send out a search expedition, under Capt. Norman, from Siberia, in case the *Dijmphna* should be in want of anything. On the other hand the Swedish-Norwegian Consul at Arkangelsk reports under date of December 13, that fishermen who had visited Waigatz Island in November last, had not seen any vessel near that island. In the last message received from Lieut. Hovgaard he expressed the opinion that the ice in the Kara Sea would

break up during the periodical storms in September and October, and enable him to reach Port Dickson, where he intended to winter. If to this is added the statements made by Mr. Leigh Smith and Sir Henry Gore Booth, as to open water north and east of Novaya Zemlya during the summer, it is not improbable that the *Dijmphna* has got free in October, and safely reached Port Dickson, or, perhaps, even Port Aktinia on the Taimur Island. Should this be the case, Lieut. Hovgaard has, no doubt, despatched a messenger to the nearest habitation, viz. Goltochicha, and thence by express to Jeniseisk, and we may therefore look forward to reassuring news from the gallant Danish explorer at the end of January or early in February.

THE results of a fourth years' observations of periodic movements of the ground as indicated by spirit levels at Secheron, are given by M. Ph. Plantamour in the *Archives des Sciences* of December 15. The curves obtained from the east-west spirit level, for the four years, are strikingly similar in the manner (pretty regular generally) in which they follow the thermal oscillations of the air. Different years show a notable difference in the epoch of maximum descent of the east side relatively to the minimum of mean temperature, and maximum rise of the same side relatively to the maximum of temperature. One is led to consider the maximum and minimum of temperature rather as accidents as regards the epoch at which they occur, and to attribute a preponderant influence to the distribution of mean temperatures during the four months November-February, and the four June-September. Probably, too, the degree of moisture influences largely the rapidity with which the deeper ground layers are affected by exterior temperature. The curve for the north-south level is also very similar to the previous ones; but has this peculiarity, that while the south side follows, in general, from October 1 to the end of September, the oscillations of external temperature (descending in winter and rising in summer) the intermediate variations of temperature have an inverse effect. The cause is at present unknown. Col. van Orff's observations at Bogenhausen reveal oscillations of the ground similar to those at Secheron, only with greater amplitude south-north, and less east-west. M. Plantamour regrets that, excepting Col. van Orff and M. d'Abbadie, no one, so far as he knows, has undertaken observations of the kind at any other station. They are easily made, and should yield important results.

NEARLY thirty years ago, Poggendorff described a "fall-machine" of his invention. Its merits, according to Herr Bauer, who spoke warmly in commendation of it at a recent meeting of philologists and schoolmasters in Karlsruhe (*Wied. Ann.*, No. 13), appear to have been somewhat overlooked. Few physical cabinets have it, and the only text-book in which Herr Bauer has found it described is that of Reis. We may state that the two pulleys over which the cord runs are at the middle and one end of a balance beam, a weight being hung from the other end (with which, and a running weight, the beam can be rendered horizontal). The machine is supplied by Herr Karl Sickler in Karlsruhe.

THE *Nation* states that from Mr. Agassiz's annual report on the condition of the Museum of Comparative Zoology it learns that it is his intention, in connection with Prof. Faxon and Dr. Mark, to issue in the Museum Memoirs a "Selection from Embryological Monographs," containing quarto illustrations derived from innumerable scientific transactions and periodicals, and serving as an atlas for any text-book on embryology. By the purchase of the large Schary collection of Bohemian Silurian fossils, and by its own rich amassing in the West and South-west during the year, the museum now contains one of the finest collections of palæozoic fossil invertebrates in existence. Mr. Samuel Garman, whose explorations for mammalian remains in the Western Territories were very successful, was led to believe from the mode of their accumulation that the cause of extinction of the more recent was "a very severe winter, much more extensive and severe" than the occasional blizzards of our time. "As if from freezing, the shafts of the larger bones are generally splintered."

WE have received the *Annuaires* of the Bureau des Longitudes and of Montsouris Observatory, both of them abounding with useful information on many subjects. Gauthier-Villars is the publisher.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus* ♂ ♂) from India, presented by Mr. J. Steel; a Black-footed Penguin (*Spheniscus demersus*) from South Africa, presented by Mr. John Wormald; a West Indian Rail (*Aramides cayennensis*) from West Indies, presented by Mr. E. H. Blomefield; an Orange-winged Dove (*Leptoptila ochroptera*) from Brazil, presented by Mr. C. A. Craven, C.M.Z.S.; a Long-eared Owl (*Asio otus*), British), presented by Mr. — Dyer; a Great Barbet (*Megalæma virens*), a Silky Starling (*Sturnus sericeus*), two Grey Thrushes (*Turdus cardis*), twelve Red-sided Tits (*Parus varius*) from Japan, a Crested Grebe (*Podiceps cristatus*), four Razorbills (*Alca torda*), a Red-throated Diver (*Colymbus septentrionalis*), British, purchased.

OUR ASTRONOMICAL COLUMN

THE GREAT COMET OF 1882.—The first determination of elliptical elements of this comet by Mr. S. C. Chandler, of Harvard Observatory, U.S., assigned a period of revolution of about 4000 years. Later investigations have diminished this period very considerably, though the length of revolution is not determined within narrow limits. Prof. Frisby, of Washington, employing observations on September 19, October 8, and November 24, gives a period of 794 years, and finds a close agreement between the position indicated by his orbit and the Cape ante-perihelion observation of September 8. Dr. Kreutz, of Berlin, using chiefly meridian observations or normal places from September 8 to November 14, gives 843 years, and finds a pretty close accordance with observation throughout this interval, thus showing no very material perturbation at perihelion passage. Further, Dr. Morrison, of Washington, founding his calculation upon positions for September 19, October 8, and December 11, finds a period of 652¼ years. But while these later computations favour a shorter revolution than was at first attributed to the comet, there remains to be ascertained to what extent the abnormal form of the nucleus since the end of September has affected the observations, and hence the deduced elements of the orbit, and a much more complete discussion of the observations than has yet been attempted, when details respecting the point of the comet observed are before us, may be required before confidence can be placed in the result of any calculation. *Primâ facie* the elements assigned by Dr. Kreutz look satisfactory enough.

In view of a possible period of seven or eight hundred years, attention may be again directed to the comet of 1106, which had several characteristics favourable to identity, though the statement in several of the chronicles (chiefly English) that at the latter part of its appearance it was seen between the north and east, is not reconcilable therewith. Pingré, advocating the identity of the comet of 1106 with the great comet of 1680 (in which he followed Halley) rather questioned the authority of the *Chronicon Alberici, Trium-fontium Monachi*, that the tail extended "below the constellation Orion," which might have been the case if the comet were identical with either of the comets of 1843, 1880, or 1882; he remarked, the monk was "ni contemporain, ni exact, ni judicieux." This subject will deserve further attention, and we may return to it shortly.

THE WASHINGTON OBSERVATORY, U.S.—The first volume of "Publications of the Washington Observatory of the University of Wisconsin" has just been issued by the director, Prof. E. S. Holden, and augurs most favourably for the reputation of this institution, which was founded within the last five years through the liberality and scientific spirit of a private individual, the Hon. C. C. Washburn. The volume is especially

valuable, from its containing a large number of measures of double stars made by Mr. S. W. Burnham during his temporary connection with the Observatory, from April 23 to September 30, 1881. The measures are contained in three catalogues: (1) a list of sixty new double stars discovered in the zone observations, chiefly by Prof. Holden; (2) a list of eighty-eight new double stars discovered and measured by Mr. Burnham; and (3) measures by the same eminent observer of 150 double stars from his manuscript general catalogue. A number of difficult objects are included in the third series. δ Sextantis was not elongated with the highest powers at the epoch 1881·34, nor did γ Coronæ show any sign of duplicity; as Mr. Burnham remarks, "it has been apparently single with all apertures since about 1871." Among the more difficult binary stars there are measures of O.Σ. 235, Σ 3123, 42 Comæ, Σ 2173, β Delphini, δ Equulei, and 85 Pegasi. There are also positions and descriptions of eighty-four red stars, of which twenty-seven are stated to be new, and a list of new nebulæ and clusters discovered in the zone-observations at the Washburn observatory.

The position of the observatory is in latitude 43° 4′ 36″·6 N., and longitude 89° 24′ 28″·3 west of Greenwich. Prof. Watson, of Ann Arbor, Michigan, was first appointed to the superintendence, but at his premature death in November, 1880, he had not been able to commence astronomical observations. Prof. Holden gives some account of the preparations he was making for scientific activity, as the only way of associating his name with the observatory.

CHEMICAL NOTES

HERR LELLMANN (*Berichte*, xv. 2835) describes an interesting case of *physical isomerism*. Dibenzyl-diamido dibromdiphenyl melts at 195°; if the liquid so produced is quickly cooled, the solid now melts at 99°, but on heating again solidifies at 125° to 130°, and melts a second time at 195°; if the substance melting at 195° be slowly cooled and then again heated, the melting point now observed is 195°.

BERTHELOT AND VIELLE (*Compt. rend.* xcv. 129) sum up the results of their researches regarding explosive waves. The propagation of an explosive wave occurs when the ignited stratum of gas exerts the maximum pressure on the adjacent stratum; increase of pressure is accompanied by increased velocity of propagation. To produce an explosive wave it is necessary that a considerable mass of gas should be employed and that the cooling by radiation and conduction should not be great; if the temperature fall below 1700°-2000°, or if the volume of the products of combustion is less than one-fourth, or in some cases one-third of the total volume of the final mixture, the propagation of the wave ceases.

ACCORDING to the experiments of M. Corne (*J. Pharm. Chim.*, [5] vi. 17) the glowing of phosphorus is due to volatilisation of the phosphorus and subsequent production of ozone by electrical energy generated by the volatilisation of the phosphorus. Phosphorus does not glow in pure oxygen at high pressures because, says M. Corne, volatilisation is impeded and at a certain limit becomes too slow to ozonise the oxygen. Gases which hinder the formation of ozone also prevent phosphorescence.

BAEYER (*Berichte*, xv. 2856) has obtained nearly pure indigo blue by acting on a solution in acetone of ortho-nitrobenzaldehyde. Acetone and nitrobenzaldehyde react to form a condensation product, $C_{10}H_{11}NO_4$, from which alkali withdraws the elements of acetic acid with production of indigo-blue, thus—

$$2C_{10}H_{11}NO_4 + 2H_2O = C_{16}H_{10}N_2O_2 + 2C_2H_4O_2 + 4H_2O.$$

J. HORBACZEWSKI (*Berichte*, xv. 2678) has obtained uric acid by heating together glycocoll and urea to 200°-230°; details of the reaction are promised.

THE working of the Food Adulteration Act for the year 1881 is considered in a Report of the Local Government Board lately issued in a Blue Book, and published in *The Analyst* (vii. 218). The total number of districts in which analysts were acting at the close of December 1881, was 260; during the year 17,823 samples were analysed, of which 2613, equal to 14·7 per cent. were reported as adulterated; in 1877, 14,706 samples were analysed, and 19·2 per cent. reported as adulterated. More than a third of the samples analysed, and more than a half of those reported against, were of milk. Birmingham still "maintains the distinction which it has for some years enjoyed, of having a larger proportion of its milk reported as adulterated than any other great town in the kingdom." The adulteration of bread and of butter seems to be steadily on the decrease; in coffee the proportion of adulteration is rather less than last year; chicory is still the commonly used adulterant. The adulteration of sugar is practically a thing of the past. More than one-fourth of all the samples of spirits examined were reported as adulterated, chiefly with water: a good deal of gin is sold containing not much more than 20 per cent. of alcohol.

PROF. HOFMANN describes in the *Berichte* (xv. 2656) a number of interesting lecture experiments. To determine that no loss of matter occurs during combustion, he employs a two-litre flask fitted with a cork carrying a small manometer, a glass tube with stopcock, and a straight piece of rather wide tubing to the under-end of which a small porcelain crucible is attached. The wide tube is closed by a cork, about half a gram of dry phosphorus is placed in the little crucible, a portion of the air in the flask is pumped out, and the flask—and also a little bit of stout copper wire—is counterpoised; the little bit of copper wire is heated and dropped down the wide tube, from which the cork is withdrawn for a moment; the phosphorus is thus ignited; after the combustion, which proceeds slowly, is completed, the flask is found to weigh the same as before; the stopcock is now opened, air rushes in, and the flask now weighs more than it did at the beginning of the experiment.

To illustrate the great difference between the volumes of equal weights of liquid and gaseous water, Dr. Hofmann employs a glass bulb of about 300 c.c. capacity, with a narrow glass tube at each end, the upper tube being fitted with a stopcock. This apparatus is supported so that the lower tube reaches to about 1 centim. from the surface of the mercury in a basin; a rapid current of steam is passed into the apparatus; after five minutes or so, when every trace of air is expelled, the stopcock is closed, and at the same moment the lower tube is pushed beneath the mercury, which at once begins to rise into the bulb; after a little time the bulb is almost filled with mercury, on the surface of which the condensed water appears as a thin layer.

VERY simple apparatuses are also described for containing considerable quantities of liquefied gases: *e.g.* SO_2: for exhibiting *quantitatively* the reactions on which the manufacture of sulphuric acid is based; and for demonstrating the law of Dulong and Petit. For descriptions of these, and for other experiments, reference must be made to the original paper.

THE HYPOTHESIS OF ACCELERATED DEVELOPMENT BY PRIMOGENITURE, AND ITS PLACE IN THE THEORY OF EVOLUTION[1]

II.

THE problem before which we are here placed may be formulated as follows:—How is it, that while the tendency to vary which obtains in all organised beings, and which forms one of the foundation stones of the theory of evolution, how is it that this tendency has exerted upon a number of living beings a so much less considerable influence than upon others, so that even in the present day numerous representatives are found of the most primitive animal groups which belong to the oldest known in the geological succession?

Still more, why are there certain genera which, since the Silurian period, appear to have undergone a stagnation in their development, in their advance towards higher differentiation, whereas within a much shorter period the whole of the living mammalian fauna has developed out of more primitive vertebrates and the important modifications have taken place among these mammalia which have finally led to the appearance of the elephant on the one hand, and of the shrews on the other?

In other words, can it be assumed that this tendency to vary could be totally and persistently neutralised by other causes amongst whole series of living beings during thousands of years, whereas during the same number of years this tendency, aided by natural selection, could lead other series of animals along roads where they have advanced with gigantic strides?

I need not remind you that this objection against the theory of

[1] By Prof. A. A. W. Hubrecht. Inaugural Address delivered in the University of Utrecht, September, 1882. Continued from page 281.

evolution, which has also been felt and combated by Darwin, was very often advanced against it, especially in the beginning. Cuvier had already reminded Lamarck that the absolute identity between the Egyptian animals, as they were embalmed three thousand years ago, with those inhabiting the same provinces in the present day, rendered untenable his ideas about gradual change and perfection of organic beings.

Huxley, to whose close reasoning powers and untiring readiness for battle the rapid progress of evolution is in a great measure due, has devoted several pages to the refutation of this objection. His argument runs as follows[1]:—

The two chief factors in the process of evolution are: the one the tendency to vary, the existence of which in all living forms may be proved by observation; the other, the influence of surrounding conditions, both upon the parent form and upon the variations which are evolved from it. Now, as often as the first factor makes itself felt, and modified forms take their origin out of a common parent form, it will depend entirely on the conditions which give rise to the struggle for existence, whether the variations which are produced shall survive and supplant the parent, or whether the parent form shall survive and supplant the variations. If the surrounding conditions are such that the parent-form is more competent to deal with them and flourish in them, than the derived forms, then, in the struggle for existence, the parent-form will maintain itself, and the derived forms will be exterminated. But if, on the contrary, the conditions are such as to be more favourable to a derived than to a parent-form, the parent-form will be extirpated, and the derived form will take its place. In the first place there will be no progression, no change of structure through any imaginable series of ages; in the second place there will be modification and change of form.

So far Huxley. No doubt but he has made us acquainted with a very reliable explanation of how the variation of any form of animals or plants may be retarded. The hypothesis of degeneration first formulated by Anton Dohrn, and afterwards warmly advocated by Ray Lankester, is no doubt of considerable importance for our comprehension of numerous lower stages of organisation in the animal and vegetable world, which may no longer be looked upon as parent-forms of more highly differentiated groups, but which, on the contrary, have in their lineage much more complicated ancestors than their own stage of organisation would appear to show. At first sight these degenerated animals show different points of similarity with animal forms, lower than those to which they are genetically allied.

So, for example, the Tunicata have for a long time been arranged amongst or close to the Mollusca, but lately-continued researches have evermore tended towards the conclusion that we have here before us the degenerate descendant of animals which had already attained the level of the lowest Vertebrates, but whose descendants, thanks to degeneration, have at present all the appearance of Invertebrates. In this way the number of lower animal types which may be looked upon as primitive, and whose persistence through geological periods gives rise to the questions as formulated above, is deceptively increased by forms, which we must remove from amongst them, and place in the vicinity of their more direct allies.

The process of degeneration is, however, confined within certain limits; it cannot do the same service towards the refutation of the objection here dealt with as can Huxley's argumentation above referred to, which is fully directed against the cardinal point, and the value of which I cannot estimate highly enough.

Still it appears to me that his explanation of the lengthened persistence of so many of the lower organised animals and plants can yet be supplemented by a new hypothesis.

To this I give the name of the hypothesis of accelerated development by primogeniture. If I have the advantage to lay it before you to-day, you will bear in mind that it has as yet only a preliminary shape, and that for its ultimate confirmation extensive researches will yet be required.

The fact is daily confirmed by continuous observation, that not only numerous vertebrates, but also very many invertebrates, can attain a very old age without the capacity for reproduction being essentially diminished. This is confirmed by the recently published researches of Weissmann[2] on the connection between the length of the reproductive period and the duration of life. We may fairly assume that all those animals attaining an old age leave issue which has been born at different periods—issue from a youthful age, which itself has again brought forth children and grandchildren, and issue from old age, which is on a level with the fourth or fifth generation of the first-born descendants. An example of old age combined with successful attempts towards reproduction is furnished by the well known sea-anemone, "Granny," which was captured in 1828 by Dalyell on the Scotch coast, and being still alive, last year gave birth to a certain number of young Actiniæ.

[1] American Addresses, p. 39

[2] A. Weissmann, "Ueber die Dauer des Lebens," 1881.

The large Tridacnas and the gigantic Cephalopods which have now and then been observed, must also have attained a considerable age; nothing authorises us to maintain that these have been infertile in all the later years of their lives. We need not stop to consider the higher groups: fishes, birds, and mammalia. They all contribute during a shorter or longer time towards the procreation of the species, and the considerable age which both fishes and birds are known to attain is the cause of a very considerable difference in age of the oldest and the youngest individuals of their own breeding. And so all of them will leave both first-born and last-born posterity. With the first-born this will in their turn be the case, so with *their* posterity, and so forth. Similarly the last-born, when they have attained maturity, will bring forth a series of descendants of very different ages; the last-born of the last-born being the final term of this series.

After centuries the effect will be this: From one pair of parents a large number of descendants will have sprung, a small number of these being the descendants in a direct line of the first-born of every successive generation; another small number being the descendants in a direct line of the last-born of every successive generation, whereas the remainder belong to intermediate stages. The first-born are separated from the primitive parent form by a number of generations, x, which is necessarily a considerable multiple of the number of generations y, which lies between the same parent form and their last-born descendants. Evidently the difference in age between the first-born descendant and his parents is a minimum, for the sole reason of his being the first-born, that between the last-born descendant and these same parents being on similar grounds a maximum. Thus, if we follow up in the direct line of descent the series of first-born of the first-born, &c., we find that the distance between two terms of that series corresponds to a much smaller number of years than the distance between two terms of the series of the continually last-born, which have always descended from last-born.

Comparing these two series simultaneously after the lapse of centuries, the series of the first-born will count numerous terms, many generations, at short distances from each other, whereas the series of the last-born will, on the contrary, consist of a much smaller number of terms, each of which is separated from its predecessor by a much more considerable distance. It is the number of these terms which in the one case I wished to express by x, in the other by y.[1]

From this fact we are led to propose the following question: Is there any reason to expect, that in the struggle for existence, the representatives of each of the two divergent series are collectively provided with different weapons? Or are both these groups quite equal to each other in the struggle?

Both observation and theoretical deduction force the conclusion upon us that a difference is indeed present. A difference, (1) in the external circumstances under which the first-born and the last-born come into existence; (2) in the internal properties and acquirements with which both series are provided; a difference which does not appear sporadically between certain representatives of both groups, but which may indeed be collectively observed between all of them.

As to the first point, the external circumstances, I call your attention to the following example, which shows how nature indeed makes a difference on a large scale in the conditions—under which she awaits the first-born and the last-born progeniture.

[1] I am doubtful whether there are indeed *first-born descendants* in the pure signification of the word, *i.e.* such which, both from the paternal and from the maternal side, count only first-born in the whole of their ancestry. However, this does not materially influence our argument. We bring together in the series of first-born all those descendants in which mixture and intercrossing with second and third births was always reduced to a minimum, whereas on the other hand, in the group of the last-born, not only those cases which are theoretically pure are brought together, but those in which the number of ancestors on both sides most closely approaches to the number of generations y, which lies between the last born *in abstracto* and the common parent form. In the majority of cases, however, intercrossing and blending will have occurred on a large scale, and the average number of generations which leads from them to this parent form may be expressed by $\frac{x+y}{2}$. The calculus of probabilities would be able to furnish us in any given case, supposing enough data are available, with the exact grouping of these numbers.

From the observations which Livingston Stone has made in 1878 in the North American institutions for fish-culture on the McCloud River, it follows that 14,000,000 eggs obtained from ripe but relatively young and smaller salmon were without exception at least one third smaller than the millions of eggs which were before obtained from older, larger salmon of the same species, and that nevertheless they developed quite normally. By these observations the fact is established that the salmon, when older, lays larger eggs than at a more youthful age, and this, more especially, is of great value for our hypothesis. Firstly, the size of the egg must influence the chances which they have for escaping or falling a prey to different voracious animals. In this respect the smaller eggs are exposed to other dangers than the larger ones. Furthermore, the relative size of the egg will, without doubt, exert a certain influence—however insignificant—upon the individual which is developed out of it.

In comparison with the larger egg of the older salmon, either the food-yolk or the formative yolk in the smaller one will be of smaller dimensions, or both together will have been reduced in size. In each of these three cases, even in the last-named, the conditions under which the smaller egg (that is to say, the whole generation of the first-born) attains its development, differ from those of the generation issued from the larger eggs, the generation of the last-born. The first-born will either be of smaller size, or because they possess a smaller food-yolk they will have to provide their own nourishment at an earlier date; or both circumstances are combined.

Nobody will deny that in each of these cases natural selection can freely come into play. In addition to this it must be remarked that however insignificant this difference in external circumstances may be its presence is nevertheless undeniable, since it reappears again with unerring certainty in every successive generation. In this way the effect can gradually accumulate, and finally the path may have been entered upon which leads to a specific differentiation of the descendants of the first and of the last born.

This having taught us that indeed the external circumstances which preside at the birth and at the growth of the first and the last born are different (at least for this species of salmon, reliable observations on a similar scale concerning other animals being for the present wanting), I must now call your attention to the second cardinal point, viz., that the internal properties and acquirements with which each of the two series of births is provided, are also different. Heredity has indeed invested them with peculiarities, part of which show themselves in their organisation, another part remaining latent, and only attaining development in following generations. Such a latent potential tendency towards eventual modification of the individual or his progeny, must needs find more numerous occasions to unfold itself in the first born, simply because these are possessed of a larger number of ancestors. On the contrary those that have a smaller number of ancestors, *i.e.* the last born, have had this occasion for development offered to them at rarer intervals. From this it follows that further modifications under the influence of natural selection will be started by preference in the different series of first born, because *ceteris paribus*, there are here more chances for the appearance of small deviations, which to a certain extent are always due to reversion to the parent forms.

And so there is reason to suppose that also the internal properties of the series of first born differ from those of the last born, in the same way as we have just defined it for external agencies. In my opinion the difference in internal structure is of greater consequence than that in external agencies, although we must at the same time acknowledge that our present methods do not allow us to test this experimentally. Only by extensive and long-continued experiments more light will be thrown on this subject. The example which was mentioned of the seventy-years-old sea anemone, which reproduced itself successfully proves that the material for similar experiments is not deficient. In the vegetable kingdom forms will certainly be hit upon which will fully reward the difficulties of the experiment.

Once a new species, modified and generally higher-differentiated, having arisen out of the first-born by gradual accumulation of the small deviations, intercrossing and bastardising with the last-born descendants of the parent form, becomes rarer, copulation taking place by preference with specimens of the same species, and only exceptionally with representatives of the species which has lagged behind in its development. For this new species the same process sets in; here, too, the first born progeniture will surpass in the course of years the last-born, and will in its turn give rise to new modifications. And so *ad infinitum*.

We now come to another important point, which is in direct connection with the question, which are the last-, which the first-born. With most lower animals—Protozoa, Cœlenterata, Echinoderms, Worms—reproduction by fission is very common by the side of reproduction. Cut arms of starfishes grow to be complete starfishes after having passed the so-called "comet" stage; certain annelids divide themselves after one of the posterior body-segments have become converted into a head; certain Nemertines break themselves into pieces under spasmodic contractions, each fragment being able to reproduce both head and tail; Amœbæ divide themselves into halves.

Now it cannot be admitted that in fissiparous reproduction, heredity can come into play in the same measure as it can in the case of sexual reproduction. It is not even possible to determine which of the two halves represents the older generation. Weissmann has lately humorously said: if we fancy an Amœba gifted with consciousness, she will think upon dividing into two, "I now bring forth a child," and there is no doubt that each half would look upon the other as the child, and upon itself as the mother. Weissmann has thus introduced the idea of the (approximate) immortality of the Protozoa, an idea which can also be adduced in favour of the hypothesis here maintained, and which at all events deserves to be mentioned by the side of the hypothesis proclaimed by Hæckel and others, viz. that the Monera living in the present day are in no genetical connection with older ancestors from earlier periods, but have come into existence by the aid of repeated spontaneous generation.[1]

The same views hold good for the self-division of worms and Cœlenterata. Here too both parts are the direct continuation of a single individual which, although dividing, does not cease to exist. Coral reefs which principally multiply by division may be looked upon in the same way.

Never, in case of fissiparous reproduction, does that mysterious potentiation take place which brings together in the egg-cell and in the spermatazoön, not only the characteristic properties of father and mother, but of whole series of ancestors; never in this case can the special process of fixation of a part of these latent forces, the process which we term *heredity*, take place to its full extent. Never can selection during embryonic and larval life, which, according to recent researches, plays a much more conspicuous part than was originally expected, favour the stability of a variation, and thus lead to modification of the species, where multiplication by division takes place.

In his chapter on pangenesis ("Origin of Species," second edition, pp. 353 and 390) Darwin too touches upon this subject, and insists upon the fact that organisms produced asexually, consequently not passing through the earlier phases of development, "will therefore not be exposed *at that period of life when structure is most readily modified* to the various causes inducing variability in the same manner as are embryos and young larval forms."

The series of generations which owe their origin to a-sexual and not to sexual reproduction, are thus in a much lesser degree liable to vary.[2] And yet a variation of some sort must always first occur, in order that natural selection, acting upon it, may finally produce a definite modification of the species. Nevertheless, fissiparous multiplication continues to play—and has always played—a very important part in the invertebrate kingdom, *by the side* of sexual reproduction. Thus the presumption is allowed, that where in the course of centuries a-sexual reproduction has been more predominant than sexual reproduction, a stagnation in development has resulted, the differentiation of those series of individuals and genera which have originated through sexual reproduction, in the meantime always continuing its regular course onwards.

Both factors—the retardation of development by a-sexual reproduction, and the acceleration of the development of the always first-born, make it very probable, in my opinion, that we have to look upon the more highly-developed groups of animals, and amongst these upon their higher-differentiated representatives, as forms which are separated from the original parent stock by a maximal number of ancestors, the number of times that a-sexual

[1] Lamarck had already, by this same assumption, attempted to overcome the difficulty.

[2] Observation tends to confirm this in a general way (*vide* Darwin, *l.c.*, p. 353.

reproduction has taken place in their ancestry being at the same time reduced to a minimum.

On the contrary, we must expect that a much smaller number of ancestors lies between the lower-developed groups and the common parent form, that a-sexual reproduction has here more repeatedly occurred, and that finally, Darwin's and Huxley's explanation, which we have above alluded to, of the non-occurrence of further modifications, may here have been realised to a greater extent.

Keeping in view the combined action of both these principles, we no longer wonder that even in the present day living representatives are found of genera which were already present in the Silurian epoch, nor that the simplest organised beings have continued to exist in that primitive form.

They are for the greater part the younger sons, and being condemned to a slower rate of development, they could not keep apace of their elder brothers. The latter, which have so much oftener passed through the improving crucible of sexual reproduction, are indebted to that cause for having become the parent stock out of which the higher and highest-developed animal and vegetable forms, now surrounding us, have gradually sprung.

THE ETHER AND ITS FUNCTIONS[1]

I HOPE that no one has been misled by an error in the printing of the title of this lecture, viz. the omission of the definite article before the word ether, into supposing that I am going to discourse on chemistry and the latest anæsthetic; you will have understood, I hope, that "ether" meant *the* ether, and that the ether is the hypothetical medium which is supposed to fill otherwise empty space.

The idea of an ether is by no means a new one. As soon as a notion of the enormous extent of space had been grasped, by means of astronomical discoveries, the question presented itself to men's minds, what was in this space? was it full, or was it empty? and the question was differently answered by different metaphysicians. Some felt that a vacuum was so abhorrent a thing that it could not by any possibility exist anywhere, but that nature would not be satisfied unless space were perfectly full. Others, again, felt that *empty* space could hardly exist, that it would shrink up to nothing like a pricked bladder unless it were kept distended by something material. In other words, they made matter the condition of extension. On the other hand, it was contended that however objectionable the idea of empty space might be, yet emptiness was a necessity in order that bodies might have room to move; that, in fact, if all space were perfectly full of matter everything would be jammed together, and nothing like free attraction or free motion of bodies round one another could go on.

And indeed there are not wanting philosophers at the present day who still believe something of this same kind, who are satisfied to think of matter as consisting of detached small particles acting on one another with forces varying as some inverse power of the distance, and who, if they can account for a phenomenon by an action exerted across empty space, are content to go no further, nor seek the cause and nature of the action more closely.[2]

Now metaphysical arguments, in so far as they have any weight or validity whatever, are unconscious appeals to experience; a person endeavours to find out whether a certain condition of things is by him conceivable, and if it is not conceivable he has some *primâ facie* ground for asserting that it probably does not exist. I say he has *some* ground, but whether it be much or little depends partly on the nature of the thing thought of, whether it be fairly simple or highly complex, and partly on the range of the man's own mental development, whether his experience be wide or narrow.

If a highly-developed mind, or set of minds, find a doctrine about some comparatively simple and fundamental matter absolutely unthinkable, it is an evidence, and it is accepted as good evidence, that the unthinkable state of things is one that has no existence; the argument being that if it did exist, either it or something not wholly unlike it would have come within the range of experience. We have no further evidence than this for the statement that two straight lines cannot inclose a space, or that the three angles of a triangle are equal to two right angles.

[1] A lecture by Prof. Oliver Lodge at the London Institution, on December 28, 1882.

[2] In illustration of this statement an article has since appeared in the January number of the *Philosophical Magazine*, by Mr. Walter Browne.

Nevertheless there is nothing final about such an argument; all that the inconceivability of a thing really proves, or can prove, is that nothing like it has ever come within the thinker's experience; and this proves nothing as to the reality or non-reality of the thing, unless his experience of the same kind of things has been so extensive as to make it reasonably probable that if such a thing had existed it would not have been so completely overlooked.

The experience of a child or a dog, on ordinary scientific phenomena, therefore, is worth next to nothing; and as the experience of a dog is to ordinary science, so is the experience of the human race to some higher phenomena, of which they at present know nothing, and against the existence of which it is perfectly futile and presumptuous to bring forward arguments about their being inconceivable, as if they were likely to be anything else.

Now if there is one thing with which the human race has been more conversant from time immemorial than another, and concerning which more experience has been unconsciously accumulated than about almost anything else that can be mentioned, it is *the action of one body on another;* the exertion of force by one body upon another, the transfer of motion and energy from one body to another; any kind of effect, no matter what, which can be produced in one body by means of another, whether the bodies be animate or inanimate. The action of a man in felling a tree, in thrusting a spear, in drawing a bow; the action of the bow again on the arrow, of powder on a bullet, of a horse on a cart; and again, the action of the earth on the moon, or of a magnet on iron. Every activity of every kind that we are conscious of may be taken as an illustration of the action of one body on another.

Now I wish to appeal to this mass of experience, and to ask, is not the direct action of one body on another across empty space, and with no means of communication whatever, is not this absolutely unthinkable? We must not answer the question offhand, but must give it due consideration, and we shall find, I think, that wherever one body acts on another by obvious contact, we are satisfied and have a feeling that the phenomenon is simple and intelligible; but that whenever one body apparently acts on another at a distance, we are irresistibly impelled to look for the connecting medium.

If a marionette dances in obedience to a prompting hand above it, any intelligent child would feel for the wire, and if no wire or anything corresponding to it were discovered, would feel that there was something uncanny and magical about the whole thing. Ancient attempts at magic were indeed attempts to obtain results without the trouble of properly causing them, to build palaces by rubbing rings or lanterns, to remove mountains by a wish instead of with the spade and pickaxe, and generally to act on bodies without any real means of communication; and modern disbelief in magic is simply a statement of the conviction of mankind that all attempts in this direction have turned out failures, and that action at a distance is impossible.

If a man explained the action of a horse or a cart by saying that there was an attraction between them varying as some high direct power of the distance, he would not be saying other than the truth—the facts may be so expressed—but he would be felt to be giving a wretchedly lame explanation, and any one who simply pointed out the traces would be going much more to the root of the matter. Similarly with the attraction of a magnet for another magnetic pole. To say that there is an attraction as the inverse cube of the distance between them is true, but it is not the whole truth; and we should be obliged to any one who will point out the traces, for traces we feel sure there are. If any one tries to picture clearly to himself the action of one body on another without any medium of communication whatever, he must fail. A medium is instinctively looked for in most cases, and if not in all, as in falling weights or in magnetic attraction, it is only because custom has made us stupidly callous to the real nature of these forces.

When we see a vehicle bowling down-hill without any visible propelling force we ought to regard it with the same mixture of curiosity and wonder as the Chinaman felt when he saw for the first time in the streets of Philadelphia a tram-car driven by a rope buried in a pipe underground. The attachment to these cars comes through a narrow slit in the pipe, and is quite unobtrusive. After regarding the car with open-mouthed astonishment for some time, the Chinaman made use of the following memorable exclamation, "No pushee—No pullee—Go like mad!" He was a philosophic Chinaman.

Remember then that whenever we see a thing being moved we must look for the rope; it may be visible or it may be invisible, but unless there is either "pushee" or "pullee" there can be no action. And if you further consider a pull it revolves itself into a push; to pull a thing towards you, you have to put your finger behind it and push; a horse is said to pull a cart, but he is really pushing at the collar; an engine pushes a truck by means of a hook and eye; and so on. There is still the further very important and difficult question as to why the parts hang together, and why when you push one part the rest follows.

Cohesion is a very striking fact, and an explanation of it is much to be desired; I shall have a little more to say about it later, but at present we have nothing more than an indication of the direction in which an explanation seems possible. We cannot speak distinctly about those actions which are as yet mysterious to us, but concerning those which are comparatively simple and intelligible we may make this general statement:—The only way of acting on a body directly is to push it behind.

There must be contact between bodies before they can directly act on each other; and if they are not in contact with each other and yet act, they must both be in contact with some third body which is the medium of communication, the rope.

Consider now for an instant the most complex case, the action of one animate body on another not touching it. To call the attention of a dog, for instance, there are several methods: one plan is to prod him with a stick, another is to heave a stone at him, a third is to whistle or call, while a fourth is to beckon him by gesture, or, what is essentially the same process, to flash sunlight into his eye with a mirror. In the first two of these methods the media of communication are perfectly obvious—the stick and the stone—in the third, the whistle, the medium is not so obvious, and this case might easily seem to a savage like action at a distance, but we know of course that it is the air, and that if the air between be taken away, all communication by sound is interrupted. But the fourth or optical method is not so interrupted; the dog can see through a vacuum perfectly well, though he cannot hear through it; but what the medium now is which conveys the impression is not so well known. The sun's light is conveyed to the earth by such a medium as this across the emptiness of planetary space. The only remaining typical plans of acting on the dog would be either by electric or magnetic attractions, or by mesmerism, and I would have you seek for the medium which conveys these impressions with just as great a certainty that there is one as in any of the other cases.

Leaving these more mysterious and subtle modes of communication, let us return to the two most simple ones, viz., the stick and the stone. These two are representative of the only possible fundamental modes of communication between distant bodies, for one is compelled to believe that every more occult mode of action will ultimately resolve itself into one or other of these two.

The stick represents the method of communication by continuous substance; the stone represents the communication by actual transfer of matter, or, as I shall call it, the projectile method. There are no other known methods for one body to act on another than by these two—by continuous medium, and by projectile.

We know one clear and well-established example of the projectile method, viz., the transmission of pressure by gases. A gas consists of particles perfectly independent of each other, and the only way in which they can act on each other is by blows. The pressure of the air is a bombardment of particles, and actions are transmitted through gases as through a row of ivory balls. Sound is propagated by each particle receiving a knock and passing it on to the next, the final effect being much the same as if the first struck particles had been shot off through the whole distance.

The explanation of the whole behaviour of gases in this manner is so simple and satisfactory, and moreover is so certainly the true account of the matter, that we are naturally tempted to ask whether this projectile theory is not the key to the universe, and whether every kind of action whatever cannot be worked out on this hypothesis of atoms blindly driving about in all directions at perfect random and with complete independence of each other except when they collide.[1] And accordingly we have the corpuscular theories of light and of gravitation; both account for the respective phenomena by a battering of particles. The corpuscular theory of gravitation is, however, full of difficulties, for it is not obvious according to it why the weight of a plate is the same when held edgeways as when held broadside on, in the stream of corpuscles; while it is surprising (as indeed it perhaps is on any hypothesis) that the weight of a body is the same in the solid, liquid, and gaseous states. It has been attempted to explain cohesion also on the same hypothesis, but the difficulties, which were great enough before, are now enormous, and to me at any rate it seems that it is only by violent straining and by improbable hypotheses that we can explain all the actions of the universe by a mere battery of particles.

Moreover, it is difficult to understand what the atoms themselves can be like, or how they can strike and bound off one another without yielding to compression and then springing out again like two elastic balls; it is difficult to understand the elasticity of really ultimate hard particles. And if the atoms are not such hard particles, but are elastic and yielding, and bound from one another according to the same sort of law that ivory balls do, of what are they composed? We shall have to begin all over again, and explain the cohesion and elasticity of the parts of the atom.

The more we think over the matter, the more are we compelled to abandon mere impact as a complete explanation of action in general. But if this be so we are driven back upon the other hypothesis, the only other, viz. communication by continuous medium.

We must begin to imagine a continuous connecting medium between the particles—a substance in which they are imbedded, and which extends into all their interstices, and extends without break to the remotest limits of space. Once grant this and difficulties begin rapidly to disappear. There is now continuous contact between the particles of bodies, and if one is pushed the others naturally receive the motion. The atoms of gas are impinging as before, but we have now a different idea of what impact means.

Gravitation is explainable by differences of pressure in the medium, caused by some action between it and matter not yet understood. Cohesion is explainable also probably in the same way.

Light consists of undulation or waves in the medium; while electricity is turning out quite possibly to be an aspect of a part of the very medium itself.

The medium is now accepted as a necessity by all modern physicists, for without it we are groping in the dark, with it we feel we have a clue which, if followed up, may lead us into the innermost secrets of nature. It has as yet been followed up very partially, but I will try and indicate the directions in which modern science is tending.

The name you choose to give to the medium is a matter of very small importance, but "the Ether" is as good a name for it as another.

As far as we know it appears to be a perfectly homogeneous incompressible continuous body incapable of being resolved into simple elements or atoms; it is, in fact, continuous, not molecular. There is no other body of which we can say this, and hence the properties of ether must be somewhat different from those of ordinary matter. But there is little difficulty in picturing a continuous substance to ourselves, inasmuch as the molecular and porous nature of ordinary matter is by no means evident to the senses, but is an inference of some difficulty.

Ether is often called a fluid, or a liquid, and again it has been called a solid and has been likened to a jelly because of its rigidity; but none of these names are very much good; all these are molecular groupings, and therefore not like ether; let us think simply and solely of a continuous frictionless medium possessing inertia, and the vagueness of the notion will be nothing more than is proper in the present state of our knowledge.

We have now to try and realise the idea of a perfectly continuous, subtle, incompressible substance pervading all space and penetrating between the molecules of all ordinary matter, which are imbedded in it, and connected with one another by its means. And we must regard it as the one universal medium by which all actions between bodies are carried on. This, then, is its function—to act as the transmitter of motion and of energy. First consider the propagation of light.

Sound is propagated by direct excursion and impact of the atoms of ordinary matter. Light is not so propagated. How do we know this?

1. Because of speed, 3×10^{10}, which is greater than anything transmissible by ordinary matter.

2. Because of the kind of vibration, as revealed by the phenomena of polarisation.

[1] To this hypothesis Mr. Tolver Preston has addressed himself with much ingenuity.

The vibrations of light are not such as can be transmitted by a set of disconnected molecules; if by molecules at all, it must be by molecules connected into a solid, *i.e.* by a body with rigidity. Rigidity means active resistance to shearing stress, *i.e.* to alteration in shape; it is also called *elasticity of figure*; it is by the possession of rigidity that a solid differs from a fluid. For a body to transmit vibrations at all it must possess inertia; transverse vibrations can only be transmitted by a body with rigidity. All matter possesses inertia, but fluids only possess volume elasticity, and accordingly can only transmit longitudinal vibrations. Light consists of transverse vibrations; air and water have no rigidity, yet they are transparent, *i.e.* transmit transverse vibrations; hence it must be the ether inside them which really conveys the motion, and the ether must have properties which, if it were ordinary matter, we should style *inertia* and *rigidity*. No highly rarefied air will serve the purpose; the ether must be a distinct body. Air *exists* indeed in planetary space even to infinity, but it is of almost infinitesimal density compared with the ether there. It is easy to calculate the density of the atmosphere at any height above the earth's surface, supposing other bodies absent.

The density of the air at a distance of n earth radii from the centre of the earth is equal to a quarter the density here divided by $10^{350\frac{n-1}{n}}$. So at a height of only 4000 miles above the surface, the atmospheric density is a number with 127 ciphers after the decimal point before the significant figures begin. The density of ether, on the other hand, has been calculated by Sir William Thomson from data furnished by Pouillet's experiments on the energy of sunlight, and from a justifiable guess as to the amplitude of a vibration, and it comes out about 10^{-18}, a number with only 17 ciphers before the significant figures. In inter-planetary space, therefore, all the air that exists is utterly negligible; the density of the ether there, though small, is enormous by comparison.

Once given the density of the ether, its rigidity follows at once, because the ratio of the rigidity to the density is the square of the velocity of transverse wave propagation, viz. in the case of ether, 9×10^{20}. The rigidity of ether comes out, therefore, to be about 900. The most rigid solid we know is steel, and compared with its rigidity, viz. 8×10^{11}, that of ether is insignificant. Neither steel nor glass, however, could transmit vibrations with anything like the speed of light, because of their great density. The rate at which transverse vibrations are propagated by crown glass is half a million centimetres per second—a considerable speed, no doubt, but the ether inside the glass transmits them 40,000 times as quick, viz. at twenty thousand million centimetres per second.

The ether outside the glass can do still better than this, it comes up to thirty thousand million, and the question arises what is the matter with the ether inside the glass that it can only transmit undulations at two-thirds the normal speed. Is it denser than free ether, or is it less rigid? Well, it is not easy to say; but the fact is certain that ether is somehow affected by the immediate neighbourhood of gross matter, and it appears to be concentrated inside it to an extent depending on the density of the matter. Fresnel's hypothesis is that the ether is really denser inside gross matter, that there is a sort of attraction between ether and the molecules of matter which results in an agglomeration or binding of some ether round each atom, and that this additional or bound ether belongs to the matter, and travels about with it. The *rigidity* of the bound ether Fresnel supposes to be the same as that of the free.

If anything like this can be imagined, a measure of the density of the bound ether is easily given. For the inverse velocity-ratio is called μ (the index of refraction), and the density is inversely as the square of the velocity, hence the density-measure is μ^2. The density of ether in free space being called 1, that inside matter has a density μ^2, and the density of the bound portion of this is $\mu^2 - 1$.

This may all sound very fanciful, but something like it is sober truth; not as it is here stated very likely, but the fact that $\left(1 - \frac{1}{\mu^2}\right)$th of the whole ether inside matter is bound to it and travels with it, while the remaining $\frac{1}{\mu^2}$th is free and blows freely through the pores, is fairly well established and confirmed by direct experiment.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following further announcements of lectures have been made:—

Prof. Humphry, Circulatory and Respiratory Systems, Jan. 25; senior class, Jan. 29; Demonstrations by the Demonstrator for Natural Science Tripos, Jan. 26; Osteology, for beginners, Jan. 17; Demonstrations for second year students, Jan. 18; Mr. McAlister will give six lectures later in the term, on the Mechanism of the Human Skeleton. Dr. Michael Foster's course of Elementary Physiology, Jan. 23; Mr. Lea, Chemical Physiology, Jan. 24; Dr. Vines, Anatomy of Plants, advanced, with practical work, Jan. 24 (Christ's College); General Elementary course, New Museums, Jan. 23, to extend over two terms, and be illustrated by demonstrations. A class for the practical study of systematic botany, Mr. T. H. Corry, assistant curator of the Herbarium, will be formed. Dr. Hicks will lecture on the Morphology of Flowering Plants, with especial reference to classification, including floral diagrams, in the Hall of Sidney College, beginning Jan. 26; Mr. Glazebrook, advanced Demonstrations in Electricity and Magnetism, Cavendish Laboratory, Jan. 24; Mr. Shaw, Demonstrations in Mechanics and Heat, Jan. 23; if more students attend than can be accommodated in the laboratory at one time, the course will be repeated on the same days. Mr. Trotter, Trinity College, Physical Optics, Jan. 25. Mr. Pattison Muir, Nonmetallic Elements, Elementary, Jan. 22, Caius College Laboratory; General Principles of Chemistry, Advanced, Jan. 23. Mr. Solly will give Demonstrations on Minerals in the Lecture Room of the Mineralogical Museum, first lecture, Jan. 22. Prof. Stuart, Jacksonian Lecture Room, Theory of Structures, Jan. 30; the Demonstrator of Mechanism, Mathematics required for Engineering, Jan. 29.

Christ's College Open Scholarships, Natural Science; E. L. Sortain, Bath College, 30*l.*; 3rd year, J. C. Bose, 30*l.*; Caius College, Natural Science, Edgworth, Clifton College, 40*l.*

MR. MARSHALL WARD is giving a course of free public lectures at Owens College, on the Nutrition of Plants.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, January.—Electric lighting in mills, by C. J. H. Woodbury.—Bricks and brick-making machinery, by C. Chambers, Jun.—Experiential principles of controlled combustion, by E. J. Mallett, Jun.—Olsen's testing machines.

Archives des Sciences Physiques et Naturelles, December 15, 1882.—Meteorological *résumé* of the year 1881 for Geneva and the great St. Bernard, by A. Kammermann.—Observations on cometary refraction, by W. Meyer.—Development of the vegetable kingdom in different regions since the tertiary epoch, according to Dr. Engler's work, by A. de Candolle.—Periodical movements of the air indicated by spirit levels, by Ph. Plantamour.—On the same, by C. von Orff.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, January 18.—Dr. Gilbert, president, in the chair.—It was announced that a ballot for the election of fellows would be held at the next meeting, February 1.—The following papers were read:—The fluorine compounds of uranium, by A. Smithells. The author has investigated the action of aqueous hydrofluoric acid upon the green uranoso-uranic oxide. He finds that a voluminous green powder, uranium tetrafluoride, is left, and that a yellow solution is formed which contains uranium oxyfluoride. The author confirms the results previously obtained by Bolton, and proves those obtained by Ditte to be erroneous.—On a new method of estimating the halogens in volatile organic compounds, by R. T. Plimpton and E. E. Graves. The authors burn the vapour of the compound in a glass Bunsen burner, the products of the combustion are aspirated through caustic soda solution, which is heated with sulphurous acid and the halogen precipitated by silver nitrate, &c., in the usual way. Good results were obtained with various liquids from ethyl bromide boiling at 39° to acetylene bromiodide boiling at 150°.—On a modified Liebig's condenser, by W. A. Shenstone. The author has slightly modified a vertical con

denser so that it can be used for prolonged digestion and subsequent distillation without shifting.—On two new aluminous mineral species Evigtokite and Liskeardite, by W. Flight.—On the volume alteration attending the mixture of salt solutions, by W. W. J. Nicol. The salts employed were NaCl, KCl, KNO_3, $NaNO_3$, $CuSO_4$ and K_2SO_4.

Zoological Society, January 16.—Prof. W. H. Flower, F.R.S., president, in the chair.—Mr. H. E. Dresser, F.Z.S., exhibited and made remarks on a specimen of *Merops philippensis*, which was said to have been obtained near the Snook, Seaton Carew, in August, 1862.—Lieut.-Col. Godwin-Austen, F.R.S., read the third and concluding of a series of papers on the shells which had been collected in Socotra by Prof. J. Bayly Balfour. The present portion treated of the freshwater shells of Socotra, which were stated all to belong to the genera *Planorbis*, *Hydrobia*, and *Melania*. Not a single bivalve was obtained. Four species were described as new, namely, *Planorbis socotrensis*, *P. cockburni*, *Hydrobia balfouri*, and *Melania sclateri*.—Prof. E. Ray Lankester, F.R.S., read a paper on the right cardiac valves of *Echidna* and of *Ornithorhynchus*. Seven additional specimens of the latter animal had been examined since the author's former paper on this subject had been read, all of which, whilst showing interesting variations, agreed in the absence of the septal flap of the right cardiac valve. This character was shown to exist also in *Echidna*, and was therefore presumed to be a distinctive feature in the structure of the Monotremes.—A communication was read from Mr. F. Moore, F.Z.S., containing the descriptions of some new genera and species of Asiatic Lepidoptera Heterocera.—A communication was read from Mr. G. B. Sowerby, jun., in which he gave the descriptions of five new species of shells from various localities.

Anthropological Institute, January 9.—Mr. A. J. Lewis in the chair.—The election of Admiral F. S. Tremlett, F.R.G.S., was announced.—Mr. Worthington G. Smith exhibited four palæolithic implements from Madras. One of them weighed 4 lbs. 7¾ oz., and the author believed that it was the second largest specimen of the kind extant.—Mr. W. S. Duncan read a paper on the probable region of man's evolution. Starting with the assumption that man was evolved from a form lower in organisation than that of the lowest type yet discovered, and that his origination formed no exception to the general law of evolution recognised as accounting for the appearance of the lower forms of life, the author said that man's most immediate ancestors must have been similar in structure to that of the existing Anthropoid apes, although it is not necessary to suppose that any of the Anthropoid apes at present existing belong to the same family as that of man. The science of the distribution of animals showed that the higher types of monkeys and apes appear to have had their origin in the Old World, the American continent being entirely destitute of them, either alive or fossil. The distribution of the greater portion of the animals of the Old World was shown to have taken a generally southward direction, owing to the gradual increase of the cold, which culminated in the last Ice Age. This migration was, however, interrupted by the interposition of the Mediterranean and other seas, and thus, although a few of these animals were enabled to journey on until they reached tropical regions, the majority were compelled to remain behind, where they had to exist under altered circumstances. The temperature was much lower, and as a result of the consequent diminution of the number of fruit forests, a change in the food and in the manner in which it was obtained by the apes occurred. A considerable alteration took place also in the manner in which they were forced to use their limbs, and it was due to the operation of these and other causes that the ape form became stamped with human characteristics such as the curvature of the spine and an increase in the breadth of the pelvis. For these reasons the author regarded the south of Europe as the part in which it was most likely that the evolution of man took place. Mr. Duncan concluded by urging the importance of forming a committee to watch discoveries bearing on this branch of anthropology.

Meteorological Society, January 17.—Annual General Meeting.—Mr. J. K. Laughton, F.R.A.S., President, in the chair.—The Secretary read the Report of the Council which showed that the total number of Fellows was 571, 47 new Fellows having been elected during the year.—The President then delivered his Address. He referred briefly to the great importance of the uniform series of observations now taken under the auspices of the Society, and proceeded to speak, at greater length, of certain other points in which the Society might, by its concerted action, further the interests of meteorological science. The first of these was anemometry, which is at present in a condition far from satisfactory. We know nothing positively either as to the pressure or the velocity of the wind; there is no exact standard instrument, and observations, whatever may be their absolute value, are not comparable one with the other. He thought that the Society might properly interfere, so far as to regulate the wide diversity amongst the instruments now used, in order that when the proper time came, and it was known what anemometer could be trusted, the older observations might be reduced. The movement of air in the upper regions of the atmosphere is not measurable by any existing method; but experiments have been made, at the suggestion of the Meteorological Council, in which the drift of the smoke-cloud of a bursting shell may be observed and measured. The observations of the barometer taken at elevated stations in the United States seem to throw considerable doubt on the received formulæ for the reduction of barometric readings to sea-level, and for the calculation of heights. When the observations extend over a long period, and are regularly taken under all conditions of weather, then no doubt the height of a mountain can be calculated with a fair approach to accuracy; but isolated observations, subject to the fluctuations of the different readings are extremely wild in their results. In the same way, the reduction of the barometer to sea-level is complicated by many discrepancies which arise between observations at the upper and lower stations, which have hitherto been ignored. It is impossible to say how far they affect the isobars on which our daily weather charts are based; but it is probable that they are at least one additional source of error and of difficulty. It is much to be wished that systematic and continuous observations at high-level stations could be taken, not only on the top of Ben Nevis, but on the top of some others of the highest peaks in different parts of the country. In this way alone, can these difficulties of reduction be cleared away.—The following gentlemen were elected the Officers and Council for the ensuing year:—President, John Knox Laughton, F.R.A.S., Vice-Presidents: Edmund Douglas Archibald, M.A., Rogers Field, B.A., Baldwin Latham, F.G.S., William Marcet, F.R.S., Treasurer, Henry Perigal, F.R.A.S., Trustees: Hon. Francis Albert Rollo Russell, Stephen William Silver, F.R.G.S., Secretaries: George James Symons, F.R.S., John William Tripe, M.D., Foreign Secretary, Robert Henry Scott, F.R.S., Council: Hon. Ralph Abercromby, William Morris Beaufort, F.R.A.S., John Sanford Dyason, F.R.G.S., Henry Storks Eaton, William Ellis, F.R.A.S., Joseph Henry Gilbert, F.R.S., Charles Harding, Robert John Lecky, F.R.A.S., Capt. John Pearse Maclear, R.N., Edward Mawley, F.R.H.S., George Matthews Whipple, F.R.A.S., Charles Theodore Williams, M.D.

EDINBURGH

Royal Society, January 15.—Prof. Maclagan, vice-president, in the chair.—In a paper on the diurnal variation of the force of the wind on the open sea and near land, Mr. Buchan gave the first instalment of the meteorological results of the *Challenger* expedition. From fully 1200 observations which had been taken, mean diurnal curves were drawn for the different oceans, from which it appeared that in the open sea no clear marked diurnal variation existed, but that near land a very evident maximum showed itself about two in the afternoon, and a much smaller maximum at midnight. Also near land the force of the wind was distinctly less than in the open sea, a fact readily accounted for by the greater friction experienced at the surface in the former case. The wind was strongest in the southern ocean, feeblest in the Pacific. Though the temperature observations had not been completely reduced, enough had been done to show that the surface temperature of the North Atlantic was subject to a very small variation of not more than ·75 of a degree Fahrenheit.—The Rev. Dr. Teape read a long paper on the Semitic and Greek article, in which he pointed out the influence of the Hebrew idiom upon the use of the Greek article, both in the Septuagint and the New Testament, and maintained, in opposition to Prof. Blackie's views, that the use of the Greek article was regulated by definite grammatical rules.—Mr. W. W. J. Nicol, M.A., B.Sc., read a paper on the nature of solution, which he regarded from the point of view of molecular attraction. Solution took place because the particles of water had a greater attraction for the particles of the salt than these had for themselves. The theory was applied to explain various facts

established by himself and other experimenters, such for example as the relation between the density of a crystal and the temperature at which it is made to crystallise out.—An elaborate experimental paper on the relative electro-chemical positions of wrought iron, steels, cast metal, etc., in sea-water and other solutions by Mr. Thomas Andrews, Assoc.M.Inst.C.E., F.C.S., was communicated by Prof. Crum Brown. The time changes in the galvanic relations were very curious, showing in some instances a complete reversal of the poles. This was regarded as probably due to the penetration of the liquid into the plates, which would thus seem to be very far from homogeneous. The experiments have evidently an important bearing on the question of erosion in sea-water.

SYDNEY

Linnean Society of New South Wales, October 25, 1882.—Dr. James C. Cox, F.L.S., &c., president, in the chair. —The following papers were read:—Description of a new species of *Solea* from Port Stephens, by E. P. Ramsay, F.L.S. This new species of sole, of which a drawing was exhibited, was proposed to be named *S. lineata*.—Contributions to Australian oology (continuation), by E. P. Ramsay, F.L.S. In this paper the author gave descriptions of the nests and eggs of nineteen additional species of Australian birds, whose nidification and oology had previously been imperfectly known.—Descriptions of Australian Micro-lepidoptera, by E. Meyrick, B.A. This, the eighth paper by Mr. Meyrick on the Micro-lepidoptera of this country, treats exclusively of the *Oecophoridæ*, a family represented in Australia by about 2000 species. Fifteen genera and 107 species are described at great length in the present paper.—Notes on the geology of the Western coal-fields, by Prof. Stephens, M.A., No. 1. This was a brief account of the Wallerawang and Capertee conglomerates and overlying coal-measures, together with some description of the Devonian beds of the Capertee Valley and Coco Creek. Specimens of *Brachiopoda* and *Favosites*, together with a large *Pleurotomaria* as well as of Porphyry and other rocks obtained from the same locality were shown in illustration of the paper.—Notes on the oyster beds at Cape Hawke, by James C. Cox, M.D., &c. This was a paper in support of the author's views, as expressed in a previous paper, of the undoubted specific difference between the drift oyster and rock oyster of our coasts.

PARIS

Academy of Sciences, January 15.—M. Blanchard in the chair.—The following papers were read:—Choice of a first meridian, by M. Faye (Report in name of Commission). This is favourable to the American proposal.—On the mechanical and physical constitution of the sun (first part), by M. Faye. He presents a *résumé* of his researches on the subject.—Researches on alkaline sulphites, by M. Berthelot.—On alkaline hyposulphites, by the same.—On complex units, by M. Kronecker.—Separation of gallium (continued), by M. Lecoq de Boisbaudran.—Table concerning the ramification of *Isatis tinctoria*, by M. Trécul.—On hydraulic silica, and on the *rôle* it plays in the hardening of hydraulic compounds, by M. Landrin. The pure silica obtained by decomposing a solution of silicate of potash with an acid, and repeatedly washing and drying at a dark red heat, he names *hydraulic silica*, and he considers it the cause of the final hardening of hydraulic mortars. The aluminate of lime cannot concur in this effect, because of solubility, but at the moment of immersion it facilitates the intimate union of the hydraulic elements, hinders water from penetrating the mass of mortar, and so aids the slow reciprocal action of the lime and hydraulic silica.—Chemical studies on maize, &c. (continued), by M. Leplay.—Treatment of typhoid fever at Lyons, in 1883, by M. Glénard. Instead of the *expectant* method, which awaits complications, combating them as they arise, the method of treatment with cold baths has been adopted in Lyons (as in Germany), with a view to preventing those complications. The mortality is thus greatly reduced (*e.g.* in the civil hospitals of Lyons from 26 to 9 per cent., in private practice to 1 or 2 per cent.).—On the proposals of M. Balbiani for opposing phylloxera, and on the winter egg of the phylloxera of American and European vines, by M. Targioni-Tozzetti. He throws doubt on the data on which the Phylloxera Commission have proceeded, in directing effort towards the destruction of the winter-egg. M. Balbiani replies at length to his arguments, none of which, he states, are new.—Treatment of phylloxerised vines, with sulpho-carbonate of potassium in 1882, by M. Mouillefert. The surface treated was 2225 hectares, on 385 properties, and a steady increase is shown since 1877. The amount of sulpho-carbonate used was 821,317 kg.; the cost varied between 200 and 450 francs per hectare; 0·05 fr. and 0·04 fr. per stock.—Observations on the subject of the Circular of the United States Government, concerning the adoption of a common initial meridian and a universal hour, by M. de Chancourtois. He advocates the adoption of a decimal division of the day and of the circle (the latter into 400 degrees, the right angle containing 100). The ancient meridian of Ptolemy, about 31·7 degrees from that of Paris, he considers the best for the initial meridian.—On the hypergeometric functions of superior order, by M. Goursat.—On Fourier's series, by M. Halphen.—On a general property of an agent whose action is proportional to the product of the quantities in presence and to any power of the distance, by M. Mercadier.—Methods for determination of the ohm, by M. Brillouin.—Reply to a note of M. Maurice Lévy.—Researches on the relative oxidisability of cast iron, steel, and soft iron, by M. Gruner. Various plates, suspended in a frame, by their four corners, were immersed simultaneously in water acidulated with 0·5 per cent. of sulphuric acid, or sea-water, or were simply exposed in moist air of a terrace. *Inter alia*, in moist air, chromate steels were oxidised most, and tungsten steels less than mere carbon steel. Cast iron, even with manganese, is oxidised less than steel and soft iron, and white specular iron less than grey cast iron. Sea-water, on the other hand, attacks cast iron more than steel, and with special energy white specular iron. Tempered steel is less attacked than the same steel annealed, soft steel less than manganese steel or chromate steel, &c. Acidulated water, like sea-water, dissolves grey cast iron more rapidly than steel, but not white specular iron; the grey impure cast iron is most strongly attacked.—On the losses and gains of nitrogen in arable land, by M. Dehérain. The losses are due not only to the exigencies of crops, but also, and for the most part, to the oxidation of azotised organic matter. When the land is not stirred, but kept as natural or artificial meadows, the combustions are less active, and the gains of nitrogen exceed the losses. Thus a farmer will more easily enrich a soil with nitrogen by keeping it in a meadow than by prodigal manuring.—Physiological action of picoline and lutidine, by MM. de Coninck and Pinet.—New experiments on irian grafts, with a view to establishing the etiology of cysts of the iris, by M. Masse.—On the solutions of continuity produced at the moment of moulting, in the apodemian system of decapod crustaceans, by M. Nevequard.

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